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1937–38 VERNON-HARCOURT LECTURE.[1]

"Estuary Channels and Embankments."

By Brysson Cunningham, D.Sc., B.E., M. Inst. C.E.

Report.[2]

)r. Brysson Cunningham commenced by stating that as the entire field f river engineering was too extensive for effective consideration in a single ecture, he proposed to deal with that part of it which was concerned rith the river-mouth and its adjacent reaches, or to use a term which ras fairly comprehensive, the Estuary. That might be designated in a road sense to signify the coastward section of a river which was to a reater or less extent invaded periodically by the sea or was subject in n appreciable degree to tidal phenomena.

After referring to the tides, he considered the physical features of stuaries and the engineering treatment appropriate to their amelioration, he main objects of which were, firstly, the regulation and improvement of he navigable channel, and secondly, the protection of adjacent low-lying ands from tidal inundation.

From the point of view of navigation, defects might arise from three auses :—

(1) a shifting, unstable channel;

(2) a shallow bed, with inadequate depth of water;

(3) a bar.

As the estuary was essentially affected by the fact that the flow of

[1] This Lecture was delivered at a meeting of the Association of London Students, nd was repeated before the Local Associations at Belfast, Birmingham, Bristol, ardiff, Glasgow, Manchester, Newcastle, Sheffield, and Southampton.

[2] Copies of the full Lecture can be obtained on loan from the Loan Library of the nstitution; a limited number of copies is also available, for retention by members, n application to the Secretary.

water was never more than for a few hours at a time moving in the sam direction, the navigable channel, unless checked by artificial means, ha a tendency to change its position and direction in accordance with th natural forces at work.

In considering the remedial measures which could be adopted to dea with that roving tendency of the channel, obviously the first step whic suggested itself was that of confining the ingoing and outgoing stream alike within the limits of a single fixed channel. In theory that wa sound enough, but the application of the principle was attended b certain risks and possibilities which had to be carefully weighed an provided for.

In the first place it was essential to have a clear conception of th great value of the tidal water on the ebb flow, and secondly the natura and inevitable accompaniment of a confinement of the main flow of th estuary within a single channel was the gradual silting up of the rest c the estuary.

In a number of cases, however, it was possible to contrive a schem of training works which would have the desired effect of confining th main stream without seriously interfering with the beneficial tidal influenc In that connexion it might be indicated in a general way that a chann should be selected for treatment which, without undue constraint, followe the normal trend of the predominant tidal flow, whether ebb or floo and the river flow, should it pursue an independent course, should b directed into the same channel. In the latter event, and perhaps in mos cases except where the maximum spring-tide flood current exceeded th combined ebb and run-off, it was desirable to commence the works at th head of the estuary and to progress seawards. The flare or divergence c the walls was a matter of importance, and the channel should be give a curvature appropriate to the local conditions.

The means whereby estuary channels could be stabilized and regulate comprised the use of groynes and the formation of training walls, with c without the aid of dredging operations, although, in a number of case dredging was relied upon entirely to maintain the navigable channe Training "walls," the main form of artifical works, were, strictly speaking not walls at all. In the majority of cases, they were simply mounds c rubble stone deposited along the sides of the channel which it was desire to regulate. In situations where supplies of rubble stone were not avai able, slag might be used, or even a mound of clay and gravel, provide that it was protected from erosion on the face by a coating of stone Walls might also be constructed of fascine work in certain cases.

One very serious possibility had to be taken into consideration pric to the adoption of a programme of training by means of walls, and that wa that every care should be taken in making the fullest investigation of th local conditions and in estimating their influence on the works proposec There the use of tidal models, for which the late Professor L. F. Vernor

Harcourt, after whom the present series of Lectures was founded, was an ardent advocate, was often of considerable assistance. Examples of improvement works on the river Seine, the estuary of the Ribble and the Whangpoo river were described.

The Lecturer then referred to cases in which the river-bed was too shallow for the needs of navigation, and said that the most speedy and direct method of achieving an increase in depth was by dredging. After dealing briefly with dredging appliances, he described the work carried out on the Thames estuary and on the river Clyde.

The third and last important defect of estuaries then came up for discussion, namely that of a bar, which was described as a ridge or narrow plateau, or even a succession of one or other or both, extending right across the mouth of a river, often more or less in a roughly circular outline, forming an elevated mound rising somewhat abruptly above the general level of the sea floor on one side and of the river-bed on the other.

After dealing with various theories put forward for the origin of bars, the Lecturer said that the most satisfactory general explanation, in his own opinion, at any rate for river-mouths in Great Britain and on the Atlantic littoral, was that which connected the bar with the phenomenon of littoral drift. That theory was not, however, of universal application, for there were bars such as those of the Yangtse river, which were largely mud, mixed here and there with patches of elutriated sand.

Difficulties met with by bar-dredging plant were described, and some figures of the work of such plant and of the quantities of materials dealt with were given.

Although training walls could be, and were, applied in outer estuaries to the formation or improvement of a navigable channel through a bar, the circumstances generally required some difference in design. In cases where a bar, or for that matter a troublesome shoal, was in close proximity to the river mouth and laid directly in the fairway of vessels making for the port, jetties or piers of various types of construction had been projected out from the shore-line, the lower part of the structures being solid work so as to confine and direct the effluent stream. Examples were given of the steps taken at the mouths of the Tyne, Wear and Tees.

Consideration was lastly given to another department of estuarial work, namely the construction and maintenance of flood-protection works, and figures were quoted relating to the Thames estuary and embankments and the Netherlands embankments. In the former case there were more than 40,000 acres of estuarial marshland in the counties of Kent and Essex requiring protection, and the embankments extended along the river on both sides for a distance of nearly 50 miles, from just below London Bridge to the sea.

The "walls," as they were termed, were embankments of earth with a core, or hearting, of puddled clay, protected on the river-side from wave-action and the wash of passing vessels by means of a revetment or

pitching stone. The provision of outlets for land-drainage purposes wa described. After dealing with the steps taken to keep the walls in good repair, and to guard against the effects of abnormally high tides, the Lecturer stated that, according to his observations, instances of wall failure, when they occurred due to tidal influence, were attributable not to overturning, but to excessive hydrostatic pressure at the inner toe of the wall, aided by infiltration of water.

The Lecture was illustrated by a large number of photographs, diagrams and plans of works carried out at various river-mouths in England and abroad.

Paper No. 5147.

"A Theory of Earth-Pressures as Applied to Retaining Walls."

By Sheikh Basheer Ahmed, M.Sc., Assoc. M. Inst. C.E.

(Ordered by the Council to be published in abstract form.)[1]

Introduction.

In the opinion of the Author, the value of the horizontal earth-pressure at the back of a retaining wall supporting a bank of earth, as calculated by most of the well-known earth-pressure theories—for instance Rankine's theory, the wedge theory, etc.—is much in excess of the true value. Designers continue to use these theories in practice, but they usually increase the value of ϕ, the angle of repose.

The filling materials used at the back of retaining walls are far from being homogeneous either in size or quality. In the Paper the Author does not enter into the more intricate questions of compression, adhesion, and non-uniform properties, but he presents a theory which conforms very nearly to the ideal conditions, and gives results which are easily applicable and are very close to the values verified experimentally.

Case 1.—*Back Fill Horizontal.*

Let ω_e denote the weight of earth in lb. per cubic foot.
" ϕ " angle of repose of the retained fill, as found by experiments, = the angle of friction.

Then $\tan\phi = \mu$, the coefficient of friction.
Let H denote the height of the wall.

Referring to *Fig. 1* (p. 366), and supposing that the wall fails at any weak horizontal section, it is clear that the wall and the wedge ABC move slightly forward first. This has been found to be the case after studying the failure of old walls, as the retained filling always adheres or clings to the back of the wall AB. Hence Scheffler's theory of earth-pressure is not justified, since the wedge ABC cannot slip downwards on the plane AB. It follows, therefore, that slipping can only take place on the plane AC at the point of failure, which is clearly causing a maximum thrust on the wall.

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[1] The MS. can be seen in the Institution Library.—Sec. Inst. C.E.

Then, considering 1 foot run of the wall,

the weight of the wedge ABC is $W = \frac{\omega_e H^2}{2} \cot\theta$,

the normal reaction on the plane AC is $N = W\cos\theta$

$$= \frac{\omega_e H^2}{2} \cdot \frac{\cos^2\theta}{\sin\theta},$$

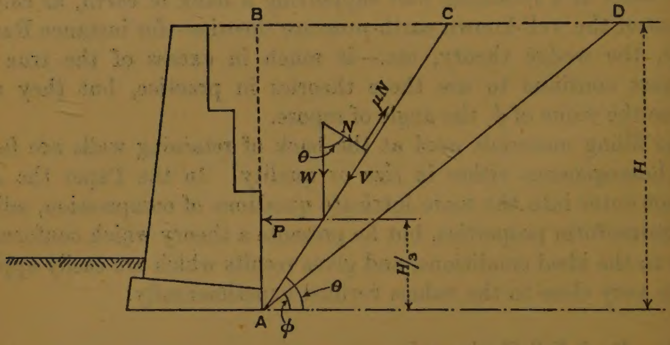
and the slipping component of W on the plane AC is $V = W\sin\theta$

$$= \frac{\omega_e H^2}{2} \cos\theta.$$

The friction on the plane AC opposing slipping is $F = \mu N$

$$= \frac{\omega_e H^2}{2} \cdot \frac{\cos^2\theta}{\sin\theta} \cdot \mu,$$

Fig. 1.



and hence the resulting slipping force down the plane AC is $S_1 = V - F$

$$= \frac{\omega_e H^2}{2} \cdot \left[\cos\theta - \mu \cdot \frac{\cos^2\theta}{\sin\theta}\right].$$

The horizontal component of S_1 is $P = S_1 \cos\theta$,

$$= \frac{\omega_e H^2}{2}\left[\cos^2\theta - \mu\frac{\cos^3\theta}{\sin\theta}\right] \quad . \; . \; . \; . \; . \; . \quad (1)$$

Now, P is a maximum when $\frac{dP}{d\theta} = 0$; hence, differentiating equation (1) and equating to zero,

$$\cot\theta = \sqrt[3]{\frac{1}{\mu}}\left[\sqrt[3]{1+\sqrt{1+\mu^2}} + \sqrt[3]{1-\sqrt{1+\mu^2}}\right] \quad . \; . \quad (2)$$

Hence θ is known, and substituting this value of θ in equation (1) the maximum value for P is obtained.

Then if $$P = \frac{\omega_e H^2}{2} . \sigma, \quad \text{(3)}$$

then $$\sigma = \left(\cos^2\theta - \mu\frac{\cos^3\theta}{\sin\theta}\right).$$

Table I gives the value of σ for various angles of repose.

TABLE I.

Angle of repose, ϕ: degrees.	5	10	15	20	25	30	35	40	45
σ	0·672	0·522	0·410	0·333	0·266	0·215	0·172	0·136	0·106
Angle of rupture, θ: degrees.	21·6	28·9	34·5	39·1	43·6	47·7	51·9	55·4	59·0

A series of experiments on brown earth, sand and granite chippings was carried out in Hong Kong in order to find the discrepancy between the experimental value and the value of horizontal pressure as derived from equation (1). Reinforced-concrete bins were used with an experimental slit in the wall at the bottom. The intensity of pressure was calculated from the beam inserted in the slit, and from this the value of σ was calculated by means of equation (3), and was compared with the theoretical value. Although the discrepancy was small, being usually less than 10 per cent., it should be mentioned that the experiments were carried out with low heads.

Case 2.—Back Fill having an Angle of Surcharge α.

By similar reasoning to that employed for dealing with horizontal back fill, the Author obtains a value for the pressure acting parallel to the surcharge (*Fig. 2*, p. 368),

namely $$P = (V - F) . \cos(\theta - \alpha)$$

$$= \frac{\omega_e H^2}{2} \cos\alpha \left[\cot(\theta - \alpha) . \cos\theta . (\sin\theta - \mu\cos\theta)\right] \quad . \quad (4)$$

For this to be a maximum, $\dfrac{dP}{d\theta} = 0$.

Hence, differentiating equation (4) with respect to θ and equating to zero,

$$\mu = \left[\frac{\sin 2\theta - \cos 2\theta . \sin(2\theta - 2\alpha)}{\sin 2\theta . \sin(2\theta - 2\alpha) + \cos 2\theta + 1}\right] \quad . \quad . \quad . \quad (5)$$

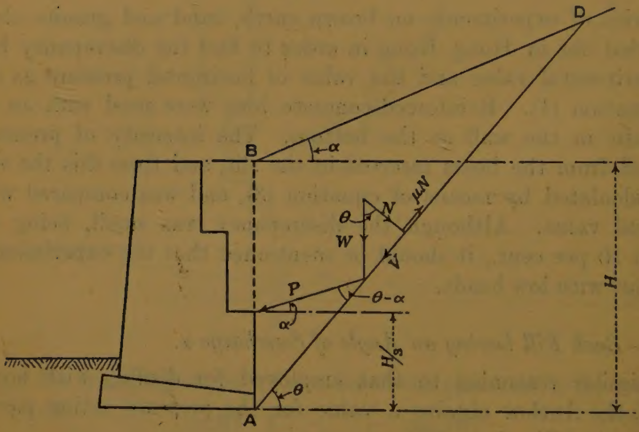
Equation (5) may be easily solved graphically, since μ is known, and having found the value of θ, the value of P may be found by substituting

this value of θ in equation (4). Table II gives the value of θ, the angl[e] of inclination of the plane of rupture with the horizontal.

TABLE II.—VALUES OF θ IN DEGREES AND MINUTES FOR VARIOUS ANGLES OF SURCHARGE.

Angle of repose, ϕ: degrees.	Angles of surcharge, α: degrees.							
	0	5	10	15	20	25	30	35
20	39 09	38 13	37 17	35 00	—	—	—	—
25	43 39	43 15	42 42	41 25	38 37	—	—	—
30	47 42	47 36	47 25	46 52	45 45	42 55	—	—
35	51 52	51 52	51 52	51 38	50 53	49 49	47 17	—
40	55 30	56 00	55 58	55 56	55 54	54 48	53 41	51 25

Fig. 2.



Design of Wall.

A retaining wall which supports a horizontal bank of filling is the case usually met with in practice. The most economic section for a retaining wall is a triangle with the virtual back vertical. This gives zero thickness of wall at the top, which is not possible in practice. An approximate rule for finding the base-width of the wall is as follows:

If B denotes the base-width of the wall,

then
$$B = H \cdot \sqrt{\frac{\omega_e \cdot \sigma}{\omega_m}} \quad \ldots \ldots \ldots \quad (6)$$

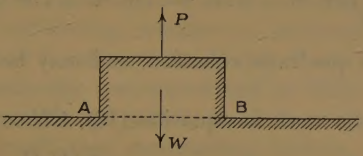
where ω_e denotes the weight of earth in lb. per cubic foot, ω_m denotes the weight of the wall in lb. per cubic foot, and σ denotes the pressure-constant, as previously described.

Case 1 of the Author's theory can also be utilized in finding the minimum foundation-depth. If h denotes the minimum depth of the foundation, B denotes the breadth of the foundation and W denotes the load on the foundation per linear foot,

then $$h = \frac{W}{B\omega_e} . \sigma^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In designing a retaining wall the resultant of W (the weight of the wall) and P (the earth-pressure) must lie within the middle third of the base. This will give a factor of safety against overturning of at least 3. By using this rule a substantial factor of safety, $\frac{\mu W}{P}$, is obtained against sliding. If the resultant force just passes through the middle third of the

Fig. 3.



base the maximum pressure at the toe of the wall-base is $\frac{2W}{B}$, and at the heel it is zero. The depth of foundation necessary is therefore

$$h = \frac{2W}{B\omega_e} . \sigma^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If the resultant passes slightly outside the middle third of the base the wall is still stable, but such a design is not considered to be sound.

Application of Cohesion to the Theory of Earth-Pressure.

Cohesion in soils and filling materials varies with the physical conditions, the effect of moisture being very important. Hence, in taking account of cohesion its minimum value must be reckoned upon.

Referring to *Fig. 3*, if an attempt is made to move the projection of earth by a force P, it is found that

$$P = W + S \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and $$S = C_1 A \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where W denotes the weight of the projection, A denotes the area of the plane AB, and C_1 denotes the cohesion per unit area.

In order to slide the projection on the plane AB, the force of friction to be overcome is

$$F = \mu W + CA.$$
$$= \mu W + \mu C_1 . A \quad . \quad . \quad . \quad . \quad . \quad (11)$$

from equation (10). Hence $$C = C_1 \mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Then, referring to *Fig. 1*, the resultant sliding force down the pla[ne] AC is $S_1 = V - F - C\,(AC)$.

$$= \frac{\omega_e H^2}{2}\left(\cos\theta - \mu\,\frac{\cos^2\theta}{\sin\theta}\right) - CH\,\text{cosec}\,\theta.$$

But $\quad P = S_1 \cos\theta$

$$= \frac{\omega_e H^2}{2}\left(\cos^2\theta - \mu\,.\,\frac{\cos^3\theta}{\sin\theta}\right) - CH\cot\theta \quad . \quad . \quad . \quad (13)$$

For P to be a maximum $\frac{dP}{d\theta} = 0$, and hence, differentiating equation (1[3]) and equating to zero,

$$-2\cos\theta\,.\,\sin^3\theta + \mu(3\sin^2\theta\,.\,\cos^2\theta + \cos^4\theta)$$
$$+ \frac{2C}{\omega_e H}(\sin^4\theta + 2\sin^2\theta\,.\,\cos^2\theta + \cos^4\theta) = 0 \quad . \quad . \quad (14)$$

Equation (14) is a bi-quadratic equation, and may be solved by the usua[l] methods.

For simplifying the work in equation (13), $CH\cot\theta = C_1\mu H\cot\theta$.

$$\simeq C_1 H.$$

Therefore, to provide for cohesion in the theory for case 1, P must b[e] reduced by the value of C_1H; that is, by the amount necessary if cohesio[n] acted on the virtual back of the wall.

Sixteen illustrations are included in the MS. of the Paper.

Paper No. 5149.

"Main Road Development in New South Wales."

By Howard Macoun Sherrard, M.C.E., Assoc. M. Inst. C.E.

(*Ordered by the Council to be published in abstract form.*)[1]

The State of New South Wales, the oldest State in the Commonwealth of Australia, has an area of 309,432 square miles and a population of about two-and-a-half million persons. The capital, Sydney, contains approximately half the population of the State. The topography consists of four defined belts of country, each parallel to the coast but of different type, namely, a narrow, well-watered and fertile coastal fringe, a belt of elevated country, a belt of undulating country of moderate rainfall, and a belt (comprising about half the area of the State) of arid country.

A main-roads authority was established in 1925, with funds provided mainly from motor-vehicle and petrol taxation. In the County of Cumberland, a relatively small area surrounding Sydney, each Municipal and Shire Council pays to the main-roads authority a rate on the unimproved capital value of the land in its area, which supplements the receipts from motor-vehicle and petrol taxation. In the country districts, however, main-road work is financed by each Council contributing a fixed proportion of the expenditure undertaken. The contributions by country Councils are as follows: State highways, nil; trunk roads, 25 per cent.; other main roads, 33⅓ per cent. In general, the main-road work is carried out by the local authorities (Councils), the main-roads authority acting as a subsidizing body. However, for various reasons the main-roads authority has assumed control of a large proportion of main roads in the County of Cumberland and of State highways in the country, and itself carries out all work on these roads.

Traffic in New South Wales is very largely motor-driven, and the general use of pneumatic tires, the strict limitation of haulage over a distance in excess of 50 miles if in competition with a railway, and the restrictions on loaded weights and on wheel-loads, enable roads to be of relatively light construction except in urban areas. There is an average of 1 motor-vehicle to each 9·66 persons.

In rural areas, the width of the carriage-way ranges from 16 feet to 20 feet. Important roads are designed on the basis of a fixed speed throughout a section of uniform topography, and horizontal and vertical curves, sight-distance, etc., are consistent with the adopted design-speed.

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[1] Type-litho copies of the complete Paper can be obtained on loan from the Loan Library of the Institution; a limited number of copies are also available, for retention by members, on application to the Secretary.

The maximum design-speed is 50 miles per hour and this has extensi application. Spiral transition-curves are used on horizontal curve Design standards are based largely on the outstanding experimental wo of the Iowa State College, U.S.A.

In rural areas, the predominant type of road-surfacing is natur gravel, which on all important routes is provided with a bitumen or t surfacing. A process known as "road-mix re-sealing" is in use f re-surfacing roads which have been previously surfaced with bitumen tar and which have become uneven. This gives very superior ridi qualities. Mechanical plant is used at all stages of surfacing-wor Another local development, the use of which has now extended outsid Australia, is use of a "drag-spreader" to spread pre-mixed bitumino macadam in one or more layers, which probably gives a higher degree smoothness than is obtained in any other type of surface or method construction. Cement-concrete surface construction conforms generall to United States practice. A local development is the use of "harsh-mix concrete for base-courses under asphaltic-concrete wearing courses. Th "harsh-mix" concrete consists of a mixture having an unusually hig percentage of coarse aggregate (for example, a mix of 1 : 2½ : 9), and rolled after placing. A marked economy is obtained as compared wit ordinary mixes. The surface is too uneven to enable it to be used as roadway, and hence it is used only as a base-course.

Maintenance of gravel roads is usually carried out by large power-drive graders, fitted with power-operated controls and mounted on low-pressur pneumatic tires. A grader of this type patrols from 75 to 100 miles o roadway, and modern units are speedy, reliable and versatile. Description are given in the Paper of methods of patching bitumen-surfaced grave roads.

Bridges are either built of steel, reinforced concrete, or hardwoo timber. The standard design-loading used provides for a vehicle weighin 18 tons, with a distributed load preceding and following it, varying with the span from and ranging from 80 to 100 lb. per square foot. Stee trusses are of the Pratt type, whilst opening spans are either of the Straus bascule type or of the vertical-lift type. Welding has been used both for trusses and plate-girder spans. Reinforced-concrete designs are based on French and German practice, and rigid frames are used ex tensively. Local hardwood timbers, principally the better classes o eucalyptus, are used for beam bridges in many cases, although timber i now seldom used in trusses. A timber working stress in bending o 3,000 lb. per square inch is used.

Reference is made in the Paper to the maintenance of the Sydney Harbour bridge and to the operation of public ferries. Tolls collected i certain cases are given.

The Paper is accompanied by twelve sheets of drawings.

Paper No. 5162.

"The Tube-Well Water-Supplies of Assam and North Bengal."

By Henry Alfred Christian Page Hetherington, Assoc. M. Inst. C.E.

(*Ordered by the Council to be published in abstract form.*)[1]

ntroduction and Geological Features.

ssam is the name given to the valley of the Brahmaputra river, which uns roughly east to west from the 96th degree to the 90th degree of longi-ude and between the 26th and 28th degrees of latitude, in eastern India. he direct distance from Sadiya to Dhubri is approximately 420 miles.

Geologically it consists almost entirely of a post-Tertiary alluvial eposit of clays and medium and fine sands. Outcrops of crystalline rocks lso occur at Tezpur, Gauhati, Nowgong, Dhubri, and other isolated areas. he sands and clays do not occur in any defined beds, and quite dissimilar onditions may be met with at sites within less than a mile of each other. o exact correlation of the strata is possible. The sands are not as a rule vell graded, a certain proportion of finer grains almost always existing ogether with the coarser particles. It is, however, generally possible to lassify the sands as fine, medium and coarse. A typical sample of sand lassified by a driller as good medium sand gave the following results vhen graded :—

Table I.

ejected by No. 20 sieve 0·75 per cent.		Passing No. 20 sieve 99·25 per cent.
" " " 40 " 63·8 "		" " 40 " 36·2 "
" " " 60 " 30·1 "		" " 60 " 5·35 "
assed by " 60 " 5·35 "		

Description : good medium grey sand.
Depth below surface : 257 feet to 271 feet.

It is from sands such as this that most of the tube-well water-supplies re obtained. The voids or pore space for medium sands, such as are ormally encountered, is between 40 and 44 per cent.

North Bengal is taken as the area lying between the Teesta and Sankosh

[1] The MS. and drawings may be seen in the Institution Library.—Sec. Inst. C.E.

rivers, and between the foot hills of the Himalayas to the north and th Brahmaputra to the south. It also consists of Tertiary deposits, thos along the hill-slopes consisting of gravel and boulders, known as th Bhaber formation, and those in the lower and more level country consistin of clays, sands, and pebble beds, known as the Terai. The boulder fo mation is by no means graded. Boulders 8 and 10 feet in thickness, mixe indiscriminately with stones, pebbles, sand and silt-like clay, form the typ of stratum most usually encountered. In this formation there is a plan of saturation, varying according to the time of the year, below which a permeable strata are water-bearing. Independent streams also flow a various depths above this saturation-level, but they cannot be relied upo as a source of water-supply as the flows are not always perennial.

In the hills to the north slates and quartzites of lower Gondwana, c Palæozoic age, have been met with near Buxa in Bengal, but of th remainder little is known until further east where sandstones and shale of Siwalik or Tertiary age are met with. In the south the hills consist gene rally of shales and sandstones of from Carboniferous to Tertiary age, togethe with outcrops of crystalline and granitic rocks of uncertain classification Coal-measures, layers of clay ironstone and ferruginous limestone als occur. The Tippam sandstone, a greenish-grey pepper-and-salt coloure rock of Tertiary age, is probably the most extensive deposit.

Type of Wells.

The wells sunk in these areas are all of the strainer type, the onl entry for water being through the strainer. A slot-opening of betwee 0·014 and 0·016 inch has been found to be the size most suitable for Assam sands, and an opening of from 0·018 to 0·019 inch for the Dooars formations

Ashford strainers of 5-inch and 7-inch nominal size are the types mos commonly used. They are made up in 8-foot lengths, each length havin an approximate total area of opening as shown in Table II.

TABLE II.

Nominal size of strainer.	Slot-opening : Inch.	Total area of opening : square inches.
5-inch (new design) . . .	0·015 to 0·016	223
7 ,, ,, ,, . . .	0·015 to 0·016	286
7 ,, ,, ,, . . .	0·018 to 0·019	318

For smaller wells Cook solid-drawn brass strainers are usually used.

Yields of Wells.

Typical examples of depths and yields of Assam wells are given i Table III.

TABLE III.

Site.	Formation.	Depth of bore : feet.	Length of strainer : feet.	Size of strainer : inches.	Standing water-level : ft.	in.	Working water-level : ft.	in.	Depression : ft.	in.	Yield : gallons per hour.
upai (Doom ooma).	Grey sand and pebbles.	229	48	5	10	6	17	6	7	0	7,900
otalguri, ariani.	Medium grey sand.	316	48	5	80	0	94	6	14	6	5,000
ilonibari, orth akhimpur.	Coarse sand.	397	48	5 (New design)	34	0	44	0	10	0	5,500
angaldai.	Medium blue and white sand.	228	48	5	15	0	22	0	7	0	6,000

The yields shown in Table IV have been obtained at different sites in he Dooars (North Bengal) and may be taken as typical of the wells in this rea.

TABLE IV.

Site.	Formation.	Depth of bore : feet.	Length of strainer : feet.	Size of strainer : inches.	Standing water-level : feet.	Working water-level : feet.	Depression : feet.	Yield : gallons per hour.
Vashabari.	Clay and boulders.	199	48	5	132	152	20	3,000
almore.	Sand and gravel.	223	32	7	136	138	2	7,000
alchini.	Sand, gravel and traces of clay.	189	48	7 (New design)	58	59	1	6,081
hooteachang.	Sand, gravel and traces of clay.	250	48	7 (New design)	142	155	13	10,800
entral ooars.	Sand and gravel.	389	56	3 (New design)	222	237	15	5,400

The water-levels given are those taken at the time of test. The levels ary almost daily and a difference of as much as 84 feet between high- and ow-water level has been measured in one year at Central Dooars. Vertical-pindle turbine pumps are usually installed in these wells, and are fitted, here possible, with variable-speed motors. The yield of these strainer-ells is directly proportional to the depression created when pumping.

The quality of the water obtained from the wells in North Bengal is

usually very good. In Assam, however, the water invariably contai[ns] iron, the content of which ranges between 0·01 and 0·9 part per 100,00[0]. The iron-content does not appear to depend on either the colour or th[e] depth of the sands. On the whole, however, it is generally found that th[e] deeper the water, the higher the iron-content.

Conclusion.

Assam and North Bengal may probably be classified among the mo[st] free-yielding areas of underground water-supplies in the world. Water i[s] available in large quantities and in most parts at comparatively shallo[w] depths. Water-levels are as a rule high, but in no case so far have pe[r]manent artesian conditions been encountered.

The Paper is accompanied by five sheets of diagrams.

CORRESPONDENCE

ON PAPERS PUBLISHED IN NOVEMBER 1937 JOURNAL.

Paper No. 5052.

"A Pre-Cast Reinforced-Concrete Underline Railway-Bridge." †

By Henry George Follenfant, B.Sc. (Eng.), Assoc. M. Inst. C.E.

Correspondence.

Mr. S. K. Ghosh, of Calcutta, observed that the Author stated on p. 31§ that some trouble had been caused by the bridge arriving $\frac{5}{8}$ inch west of its proper position, due apparently to "side-slip." It might be of interest to state that a similar side-slip had occurred when a steel bridge of 100-foot span and 100 tons weight was being skidded laterally between greased plates a distance of about 27 feet, to serve as a diversion and to make room for a new reinforced-concrete bridge to be built in its place. That work had been undertaken by his firm for the Public Works Department of the Government of Bengal. The procedure adopted for hauling had been practically the same as in the case described by the Author; namely, that the pull was given by two hand-winches, placed a certain distance away.

During the shifting, the bridge had been found to have a tendency to slip sideways. As soon as that had been found out, the haulage-ropes had been deflected slightly sideways by passing them through a pair of snatch-blocks fixed at suitable points in between the winches and the bridge, in order to give an inclined pull in the direction opposite to the slip. Actually, two pairs of such snatch-blocks had been fixed, one on each side of the direct line of pull of each rope. That had helped to correct side-slip in either direction, and the bridge had arrived in its proper position without any difficulty.

The Author observed that the Correspondence on his Paper did not call for any reply.

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† Journal Inst. C.E., vol. 7 (1937–38), p. 25 (November 1937).

§ Page numbers so marked refer to the Paper (Footnote (†) above).—Sec. Inst. C.E.

Students' Paper No. 937.

"Modern Swimming-Pool Design." †

By Edwin Lomax, Assoc. M. Inst. C.E.

Correspondence.

Mr. F. M. G. Du-Plat-Taylor observed that, as swimming-poo sometimes cost as much as £20,000 or more, their design was a subje which might well receive more attention. All the pools illustrated ha vertical walls, but considerable economy could be realized by forming sid slopes in the floor at the foot of the walls, and going down to the full dep required. That arrangement reduced the height of the vertical wal and was unobjectionable, provided that the slope did not project too f from the face-line of the wall.

In regard to the question of under-pressure on the floor of a poo when it was empty, he had adopted the expedient of inserting in th floor small gunmetal relief-valves in connexion with a system of shallo trenches filled with loose stone beneath the floor. The valves were the ordinary poppet type about 2 inches in diameter, working in gun metal seatings and covered with removable gunmetal gratings. The could be cleaned and ground-in whenever the pool was empty. No leaka in the reverse direction had occurred with those valves when the pool wa full. The use of the valves enabled the floor to be designed with sing reinforcement.

In his opinion rendering applied in the usual way was not a satisfacto finish for the surfaces of a swimming-pool, or for any tank. It generall developed crazing, and eventually disintegrated. The surface lay should be applied before the under layer was completely set, or should b deposited simultaneously with the under layer.

The system mentioned on p. 85 §, which was known as the "Impervio Pre-cast system," was wrongly marked in *Fig. 13* (p. 85 §) as limited to height of vertical wall of 8 feet; it could be adapted to a height of 10 fee By that system the polished or other ornamental surface for the wall wa applied to the faces of the blocks in the factory.

The system of chlorine-sterilization was rapidly giving place to oth methods, owing to the difficulty of always ensuring uniform dosage ar the consequent unpleasant taste and smell.

That difficulty was, however, avoided by the "Chloramine" syste In his view a system devoid of any unpleasant taste or smell was essenti and he had found ozone to be the most satisfactory in that respect, pr

† Journal Inst. C.E., vol. 7 (1937–38), p. 77. (November 1937.)

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Se Inst. C.E.

vided that the gas was discharged directly into the water of the pool itself.

Mr. Ralph Marshall pointed out that, although the Author had referred to Continental swimming-pools and to wave-making machinery, there was no mention of what he thought was the first swimming-pool with wave-making machinery to be constructed; that pool belonged to a hotel in Budapest, and was in full operation in the summer of 1928. The pool was in connexion with the thermal bath established at the hotel, and was of the rectangular open-air type. At the shallow end was a sand beach with artificial rocks on which the waves spent themselves. He had been unable to see the wave-making plant, but from the way that the waves would originate at the deep end either at one side only or from each corner, with a period of from 2 to 3 seconds between them, it appeared as if the plant were of the plunger type, or, if it were of the hinged-shutter type, that the shutters were not all swung backwards and forwards at the same time. The normal waves produced were very strong, and the pool was supplied with life-belts around the sides, only strong swimmers being able to remain in the deep portion while waves were being produced. The artificial rocks gave a most pleasing appearance to the beach as the waves broke on it, but they might have since been removed as he saw bathers receive cuts through being swept on to them. The wave baths at Luna Park, Berlin, were, he believed, constructed shortly after the baths at Budapest, so that Hungary deserved mention as a pioneer, if not the originator, of wave baths.

Mr. John Pollok, of Nairobi, sought further information on certain points. *Fig. 12* (p. 85 §) showed a copper-strip construction-joint, and the remark was made that that type of construction-joint was used in order to "bond the work together." He was under the impression that construction-joints were used, not to bond the work together, but rather to allow free movement of the concrete; the argument being that it was better to localize any movement of the concrete rather than to allow contraction- or expansion-openings to appear haphazard in the wall. If his view of those construction-joints were correct, then the horizontal reinforcement bars should not be carried across the joints, but as far as he could ascertain it was a common practice to carry the horizontal reinforcement across those joints.

The Author laid stress on the necessity of well draining the site, and also recommended that friction between the foundation and the floor-slab should be eliminated as far as possible; but were those recommendations really carried out in general practice? A most important point, in Mr. Pollok's opinion, was the laying of the floor-slab. The Author recommended a layer of bitumen at each construction-joint; Mr. Pollok agreed that some form of watertight joint was necessary, but many

§ *Ibid.*

engineers seemed to be satisfied with "buttering" the joint with 2-to cement mortar, even with a head of 15 feet of water. With a large floo area that form of joint was not likely to prove very efficient.

The copper-strip vertical joint in the walls and the omission of proper watertight seal at the floor-joints were the weak points commo to reinforced-concrete water-tanks, whether they acted as storage-reservoi or as swimming-pools.

Mr. E. F. Roberts, of Dunedin, N.Z., referring to p. 83 §, where it wa stated that "possible shrinkage of the earth backing makes it advisab to design the reinforcement for both cases," asked whether it would not b possible, in cases where suitable material was available, to load the wa with sand backfill? In that case shrinkage would not occur and later pressure would always exist. Mr. H. J. Nichols, in a Paper † entitle "Frame Arch Spans for Railway Loading," suggested the use of sand i a similar way behind the vertical walls of rigid-frame bridges.

The Author, on p. 84 §, made the following statement: "Owing to th large volume of concrete there will be a correspondingly large shrinkage." That statement appeared to require modification, shrinkage being depen dent more on length than on cross-sectional area.

Amplification of the description of joints in the floor-slabs would b appreciated. What was the purpose of the rubber hose in the left-hand diagram of *Figs. 14* (p. 87 §)? At a point where four slab corners met, ho were the joints of the bent copper strips effected? Were they joined? I so, information on the matter would be appreciated, as such attempts as h had seen to join those strips at that place appeared to be too rigid to allow any movement, and liable, if the strips were securely bonded to th concrete, to cause cracking of the corners of the slabs. Would the Autho give a specification for suitable bitumen for those joints?

The Author, in reply, agreed with Mr. Du-Plat-Taylor that the desig of swimming-pools was a subject which might well receive more attention particularly in view of the National Fitness Campaign and the necessity for designing such buildings as central features of comprehensive scheme for physical training and recreation. The question of effecting economie by forming side slopes in the floor was a matter for individual design that suggestion could only be adopted with safety at the deep ends of large open-air pools. With reference to the question of whether or not rendering was a satisfactory finish for the lining of a swimming-pool, the Autho pointed out that such a finish was by no means ideal, but, where initia cost was a primary consideration, it would be permissible to use the method In his opinion, it could be expected to last at least 20 years.

Mr. Pollok's remarks regarding the copper-strip construction-join were quite correct. Such a joint would "link the work together" rathe

§ *Ibid.*
† Indian Railway Technical Paper No. 294. Calcutta, 1935.

han act as a bond. His remarks as to reinforcement bars not being carried across joints, however, were hardly applicable to swimming-pool design, as such joints were used in mass concrete walls, and were not necessary in thin reinforced-concrete walls used in connexion with swimming-pools. The Author's recommendation that friction between the foundation and the floor-slab should be eliminated as far as possible was stressed in view of the fact that such conditions were not found in general practice. The absence of such conditions was one reason for the failure of the floor-slab.

The possible use of sand backfill, suggested by Mr. Roberts, was quite sound, but it was hardly applicable to a small swimming-pool design, as the actual saving would be very slight. The Author could not agree that the shrinkage of concrete was entirely dependent upon length rather than volume. The rubber hose referred to in the left-hand diagram of *Figs. 14* was intended to allow movement between the floor- and base-slabs whilst still maintaining a water-tight compressible jointing material. The joints of the bent copper strips were so arranged that no four corners met. The joints were thus of T pattern, and no attempt was made to join the strips rigidly. It was, however, possible to effect a floor-slab junction with bent copper strips by arranging one continuous joint to base over the other continuous joint at different levels in the slabs. No detailed specification of the bitumen was available for construction-joints, as the Author considered that any bitumen suitable for use in the joints of reinforced-concrete road-slabs could be used for the purpose, the temperature-range being so low, if arrangements were made to protect the pool when empty against overheating in hot weather.

Paper No. 5137.

"The Reduction of Carrying Capacity of Pipes with Age." †

By CYRIL FRANK COLEBROOK, Ph.D., B.Sc. (Eng.), Stud. Inst. C.E., and Assistant Professor CEDRIC MASEY WHITE, Ph.D.

Correspondence.

Mr. M. R. Barnett observed that the theoretical reasoning in the Paper was based principally on the experiments made and the conclusions arrived at by Mr. A. A. Barnes, M. Inst. C.E., in his Paper on "Discharge of Large Cast-Iron Pipe Lines in relation to their Age." * The Authors of the present Paper characterized that work as "probably the most reliable of the ageing experiments carried out in Great Britain."

† Journal Inst. C.E., vol. 7 (1937–38), p. 99 (November 1937).
* Minutes of Proceedings Inst. C.E., vol. ccviii (1918–19, Part II), p. 1.

Mr. Barnett had been Resident Engineer on the Thirlmere aquedu during the original construction, and again on the work of laying t second line of pipes. During both periods he had had the supervision the Keer siphon (40-inch and 44-inch diameter), which lengths of pi formed the principal basis of the conclusions arrived at by Mr. Barne He hoped that it might not be too late to place on record some of t salient points which had come to his knowledge during those two perio on the Thirlmere aqueduct, which had a very material, if not a vita effect upon the theory on which the conclusions were arrived at by M Barnes.

In an earlier Paper ‡ entitled "Deposits in Pipes and other Channe Conveying Potable Water," Professor Campbell Brown had set out in most interesting, succinct, and convincing form his experimental research and the results which he had proved therefrom, bearing upon the questio of the causes which affected the reduction in the delivery of the 42-inc diameter main from Vyrnwy to Liverpool after it had been in use f about 13 years, as referred to on pp. 45–46 of the Discussion on his Paper Professor Campbell Brown had proved most conclusively that incrustati was neither the only, nor in some cases the chief, factor in causing th reduction in the delivery of iron pipes. In his Paper he classified th deposits into three classes :—

I. Incrustations on unprotected or imperfectly-protected iro pipes.
II. Deposits on the inner surface alike of iron pipes, whether pro tected or unprotected, and of culverts, rock tunnels and othe channels : the deposits depending in their nature on the com position of the water, and occurring over the whole of th surface covered by the water.
III. Accumulations of débris in inverts, on hollows and irregulariti in the water-channels, and in the *culs-de-sac*.

He gave the following description of the first class of deposit :—

"Incrustations are formed by the corrosion of iron pipes, valves and othe ironwork, where the inner surfaces are not protected by a layer of pitch, or whe the protecting layer is imperfect.

"These incrustations begin as minute projections dotted all over the pipe in very varied numbers—sometimes in rows, sometimes in an irregular manne In form they resemble the limpets to be found on a rocky sea coast ; they ar more or less conical in shape, often with a steep or more sloping side, and gro by addition of concentric layers. The steep side faces the current. The increase in size and in number, until in badly-protected pipes they ultimatel become confluent, and form a coating of considerable thickness. This kind incrustation may be nearly uniform from the commencement, in which cas nodules are not then seen ; but the nature of the incrustation is the same.

‡ Minutes of Proceedings Inst. C.E., vol. clvi (1903–04, Part II), p. 1.
|| Footnote (‡) above.

pipe of small calibre is sometimes completely blocked by the incrustation. In large pipes the thickness of the deposit does not appear to be unlimited. In old pipes, when the incrustation is about 1 inch to 1½ inch thick it does not seem to increase, possibly owing to the density of the incrustation acting as a protection; but if the incrustation is removed, or if the conical tips of the 'limpets' are broken off, it begins to grow again with renewed activity. For this reason it is inadvisable to use a scraper for removing the nodules or incrustations. The obstruction is only temporarily removed, and if the use of the scraper is continued, the life of the pipe is greatly reduced by the more rapid corrosion of the iron."

Mr. Barnett repeated that description because it was a most accurate account of the manner in which such incrustations were formed, and of the effect that they had upon the pipes, as he had actually observed during the long period in which he had been supervising the laying of water-mains of the largest and smallest diameters.

With reference to the second cause of reduction in the delivery of pipes, Professor Brown stated that a black slimy lining formed wherever the water was in contact with the sides of the pipes, culverts, or tunnels. The deposit contained a good deal of iron, but the iron was not derived from the pipes, because the deposit occurred on protected as well as on unprotected pipes, and on stonework, woodwork, brick and rock surfaces, where the water had never been in contact with pipes, nor with ironwork of any kind. The deposit lessened the flow of water, not only by diminishing the sectional area of the pipes, but also by greatly decreasing the velocity of the water. That layer had been called "peaty," but Professor Brown stated that he had never been able to find any particle of peat in it; further, the slime did not accumulate for several years, whereas peat would begin to be deposited in the first year. He also proved that the iron came from the water, and not from the iron of the pipes, as similar slime was found in rock tunnels before the water had been in contact with iron, and the slime was found as plentifully on the sides of pipes completely protected by a coating of pitch as on uncoated iron. Further, the water always contained iron when slime was found, whilst water from similar gathering-grounds, but containing little or no iron, did not produce that kind of slime; again, in a very long length of pipe, iron was found to predominate in the first few miles, while the quantity of iron decreased, and manganese increased, in the last few miles. In replying to the discussion on his Paper, Professor Brown stated that the choking of the Vyrnwy pipes was not due to such incrustations as those described. "Limpets" were dotted over them, but they did not seriously impede the flow, the impediment being due to the process of growth of the deposit that he had described in his Paper.

Mr. Barnett pointed out that it was to be noted that where there was iron in the water the black slime formed with like facility in concrete, masonry, and other forms of aqueduct, and not only in pipes. The effect of the slime in retarding the flow of the water in the concrete culvert

portions of the Thirlmere aqueduct, as well as in all the siphons nor of Beehive and Keer siphons, had, therefore, to be ascertained with t greatest accuracy, before an accurate gauging could be made of the tr net quantity of water flowing and the rate of flow at the inlet end of, sa Keer siphon. Unless allowance were made for all retarding effects the water above the section selected for so important a series of tes it seemed that a comparison between the theoretical quantity and t actual quantity (as measured), was not sufficiently reliable to form t basis of a universal law of the delivery of large cast-iron pipes in the later life. It would also be imperative that special allowance should made for the retardation of the water in the many curved portions of th concrete aqueduct. Two of the most noticeable instances of qui changes of direction in the line of the aqueduct occurred north of th Keer siphon. The first was at the junction of the south well of th Scandale siphon with the culvert in continuation thereof; there w a horizontal right-angled bend in the pipe, followed by the right-angle vertical bend delivering the water into the south well, and a very co siderable throttling of the flow was bound to be caused by such quic changes in direction. Again, at the south well of the Lupton sipho the water from the pipe-line, immediately it entered the concrete culver had to turn a sharp curve of 86 degrees to the left, followed almost at onc by another curve of 33 degrees to the right. When the coating of slin had had time to form over the water-surface of the culvert, those abru changes of direction were bound to have had a serious effect in retar ing the velocity of the water, and therefore the discharge. Tha such an effect existed in the concrete-culvert portions of the Thirlme aqueduct seemed to be evident from the following facts: in 1903, whi Mr. Barnett had been engaged as Resident Engineer in the laying of th second line of pipes from Thirlmere to Manchester, he had also been supe intending the repairs of a section of the concrete aqueduct at Hutto Roof,[1] immediately to the north of Keer siphon. That had necessitate the formation of a new surface to the concrete culvert for a length of abo 3 miles. A thick coating of slime, similar to that described by Profess Campbell Brown, had been quite evident on the water-surface of th culvert, as well as on the internal surface of the first line of pipes, whic had been brought into use 9 years previously. When the new surfa of the culvert had been completed, it had been observed that the san growth had immediately begun to develop, and it had been most interes ing to watch it spread again over the new surface of the culvert, exact as described and illustrated by Professor Brown in his Paper.[2] The ra of the growth of that slime would not justify a reduction of 5·2 per cen in the flow of water in 1 month, or even in a year or two.

[1] M. R. Barnett, "Repairing a Limestone-Concrete Aqueduct." Minutes Proceedings Inst. C.E., vol. clxvii (1906–07, Part I), p. 153.

[2] Footnote (‡), p. 382.

The obvious and only effective remedy for that cause of reduction in the delivery of water-mains was to have the water treated chemically and filtered at the source, before it entered either a concrete culvert or a line of pipes.

Professor C. W. L. Alexander, discussing the Paper [1] by Mr. Barnes, had said that it was hard to believe that a 44-inch main suffered a reduction of 13 per cent. in its capacity in 13 months. The reduced capacity was due to the joint effect of reduced cross-sectional area and increased friction. If the cross section were supposed to remain the same, then, with 13 per cent. reduction in capacity, it would mean that 37 per cent. of the available head was being spent in overcoming fresh resistances that had arisen inside the pipe in 13 months. If the velocity were supposed to remain the same, then, with the 13 per cent. reduction in capacity, the diameter had diminished from 44 to 42 inches. It was evident, by considering the question in that way, that, while the decrease in capacity was due to both causes together, the amount was so large as to require rigid proof that some other factor was not at work. Mr. Mallett cited a case where a forgotten meter caused a diminution of 28 per cent. in a length of main. It was for that reason that Mr. Barnett put forward a number of circumstances and facts within his personal knowledge, which very materially affected the premises upon which the theoretical reasoning and conclusions of Mr. Barnes, and consequently, the reasoning and conclusions arrived at by the Authors of the present Paper, were founded.

In any aqueduct like that from Thirlmere to Manchester, with so many varying conditions both in the concrete-culvert portions and in the cast-iron siphons, it would be a most complicated undertaking so to balance everything as to ensure exact uniformity of flow from one end to the other of the 96-mile length of the aqueduct. Each section of culvert and each length of siphon-pipe had to be tested in relation to its individual details. When there was a depth of 19 inches of water flowing in the concrete culvert, 10 million gallons per day were intended to be sent through each siphon [2]; instead of that, however, in the last 4½ months of 1902 the depth of water in Keer north well had varied from 4 feet 8½ inches to 6 feet 3½ inches. That was not much less than the depth of the water shown on the original drawings for the delivery of the full 50 million gallons per day when five lines of pipes were in use. That variation of 19 inches in the depth of water in the north well was equivalent to the depth shown on the original drawings for the delivery of the first 10 million gallons per day. It was evident, therefore, that the quantity of water delivered for the Keer siphon to pass was constantly

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[1] Footnote (*), p. 381.

[2] *See* Fig. 10, Plate 1, of Mr. G. H. Hill's Paper, "The Thirlmere Works for the Water-Supply of Manchester." Minutes of Proceedings Inst. C.E., vol. cxxvi (1895–96, Part IV), p. 2.

varying, and that fact alone rather detracted from the reliability of any data based upon the figures derived therefrom.

Why was that the case? Obviously the pipe had not been passing the water at the required rate. The water was given a complicated task to get from the culvert to the siphon-pipe. It had, firstly, to flow through the 8-inch space between the bottom of the bell-valve and the base-casting in the floor of the well, and then followed a right-angled bend and a vertical drop of 3 feet 6 inches; next there was an abrupt horizontal change of direction, leading through an 8-foot 10-inch length of tube (5 feet 3 inches by 2 feet 4 inches); the discharge from that tube was horizontally through seven circular orifices 1 foot 3 inches in diameter, and vertically between the float and the 4-foot 8-inch diameter opening into the float-well. From that very small well the water had to find its way horizontally into the siphon-pipe by a bellmouth inlet-pipe, but the form of that bellmouth pipe did not conform to the correct theoretical streamlines for such inlet-pipes. All those changes of direction were as abrupt as possible and no attempt had been made to facilitate the flow of the water, by streamline curves. In addition to that direct obstruction of the flow through the castings, there was a medley of castings occupying a considerable portion of the floor-space of the inlet- or valve-well. How much of the additional head was taken up in overcoming the obstructions to the flow detailed above? Another cause of the additional head required was that there was a reflux valve of a very heavy and obstructive pattern installed on the southern leg of that siphon and 6 inches of head were absorbed in forcing the water through that valve. In one siphon it had been found that the heavy horizontal cast-iron doors of the reflux valve had been removed, because they had become fast with corrosion. There was also a less serious cause of increased head in the right-angled curved bellmouth outlet-pipe which delivered the water into the south well. The most serious loss of head within the siphon-pipes, however, was caused by the large number of curves, vertical and horizontal, which had been used for the changes of direction in the line of pipes. Those curves were formed by the use of double-socket bevel castings of angles ranging from 1 degree to 10 degrees. They were used, singly or in combination, to turn any angle, by joining them with spigot pipes 3 feet in length. In the Keer siphon, one hundred and eighty-two of those bevels were used, totalling, with the short C-pipes, 325 linear feet of curved pipe-line. Besides being a very expensive method of forming curves on a line of pipes, that method was also open to the objection that it formed an unnecessary obstruction to the flow of the water, which had to pass over the corrugations caused by the number of the joints, and thus reduced to an appreciable amount the delivery of the main.

In a main laid during recent years, in which the line of pipes had been made as straight as possible, the few easy curved pipes which could

not be avoided had entailed an additional head of 5 inches in a length of 3 miles of main. From that it might be inferred how much additional head would be involved by the 325 linear feet of curved pipe employed on the Keer siphon.

From the experimental data a curve had been produced by Mr. Barnes upon which he had based a Table of the probable percentage of diminution up to the age of 100 years.[1] It was to be regretted that Mr. Barnes had not given some details of how he had arrived at the amount of reduction in 1 month and in the periods of a few years included in his Table. Mr. Barnett had remained on the Thirlmere aqueduct for some considerable time after the water had been flowing to Manchester both through the first pipe and also through the second line of pipes, and he had heard of no accurate early measurements of the actual delivery of the first or second pipe-line. A suggestion that a venturi meter should be inserted in the longer siphons of the second pipe-line had not been adopted on account of the extra loss of head that it would have involved.

The following figures were taken from Table II in Mr. Barnes's Paper [1] :—

Age.	Actual reduction in delivery : per cent.	Assumed reduction in delivery : per cent.
1 month .	5·2	—
4 years . .	21·7	—
15 ,, . .	35·4	—
50 ,, . .	—	55·3
100 ,, . .	—	71·4

That the growth of incrustation or the development of slime should be so very rapid in a new pipe as to reduce its flow by 5·2 per cent. in 1 month seemed to be scarcely credible. To any experienced pipe-layer, it was obvious that the cause of such rapid diminution in the delivery had to be looked for in an entirely different direction. Again, it seemed an extraordinary assumption to make that, because pipes of small diameter became so choked, cast-iron pipes of such large diameter as 40 and 44 inches would also be so greatly reduced in 100 years as to be capable of delivering only 28·6 per cent. of their calculated capacity. The well-known fact that incrustation did not continue to grow indefinitely until it choked the pipes of larger diameter, but remained stationary after reaching from 1 to 1½ inch in thickness, had evidently been ignored. Another very significant fact had also been overlooked ; namely, that before the inside of a pipe 3 feet or more in diameter could become so nearly full of "carbuncles," the whole of the "goodness" of the metal of a cast-iron pipe would have been used up to produce the necessary volume of incrustation, and none of the

[1] Footnote (*), p. 381.

effective metal would be left to resist the internal pressure of the water. Neither could the growth of the black slime continue indefinitly, because when a certain thickness of that coating had been attained, the viscosity of the slime would prevent it from remaining in situ, so that it was bound to drop off and to become a deposit in the bottom of the main, and hence it would have to be removed by brush or by scouring.

The periods of 4 years and 50 years had been selected from the Table of percentages of loss for the purpose of comparison with corresponding periods in the life of pipe-lines within Mr. Barnett's knowledge. The last trunk main laid under his supervision, after having been in use for 4 years, was still capable of delivering 50 per cent. in excess of (not 21·7 per cent. less than) the calculated capacity. The pipe was as clean and as free from rust as when it had been laid 5 years previously. Another trunk main, which had been laid 50 years previously, was still delivering the same quantity of water as it had done 20 years before, when it had first come under his control. According to the figures given in the Table of reduction, the delivery should have decreased by an additional 9·5 per cent. in the period from 30 to 50 years; the main was then, however, actually delivering 98 per cent. of the amount which it had been originally designed to convey, instead of the 55·3 per cent. which it should have been discharging according to Mr. Barnes's Table. It had been cut into and did not show any serious corrosion—there having been only a few very small "limpets" at one side—after 52 years' service for an especially acid water.

Again, quoting from Table I of Mr. Barnes's Paper,[1] the discharges at various ages had been:

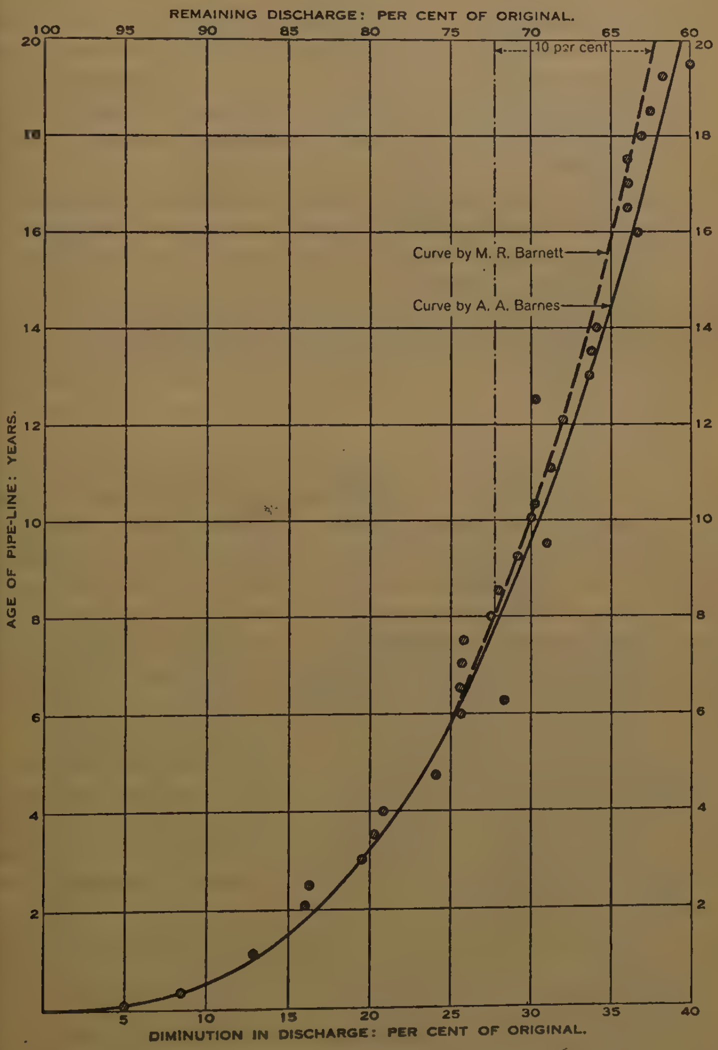
Discharge from 40-inch diameter pipes: gallons per day.		Discharge from 44-inch diameter pipes: gallons per day.	
When new	12,408,000	When new	15,805,000
19½ years old	7,445,000	9½ years old	10,905,000
Difference . . .	4,963,000	Difference . . .	4,900,000

It was rather significant that the reduction in discharge of the second line of pipes, laid on the same length and alongside the first line of pipes (although they were of larger diameter), should have been almost exactly the same in 9½ years as that of the first line in 19½ years. That fact alone appeared to upset the whole theory upon which Mr. Barnes based his conclusions. It seemed to uphold the belief that the rapid diminution of delivery during the early years in both of those mains was due to the other causes suggested above, and not to incrustation, etc., on account of ageing of the mains.

[1] Footnote (*), p. 381.

The conclusion to be drawn from those remarks was clearly that, when all the other causes of the reduction in the delivery-capacity of the mains

Fig. 11.



had been separated from the total reduction due to "ageing," the remainder might then with greater confidence be set principally against the formation of the black slimy viscous growth on the interior surface of the pipes, or against incrustation of the pipes. The first 6 years of reduction in delivery capacity might be attributed to the accumulation of the causes suggested

with regard to the concrete culvert and of the other more physical cause referred to in the siphon-pipes.

In the light of the facts set forth above Mr. Barnett would suggest tha the Authors of the present Paper should take the curve used by M Barnes, for the diminution in discharge over a period of years, and repro duced in *Fig. 11*, as amended by the dotted line, as the basis for the theory on "The Reduction of Carrying Capacity of Pipes with Age." The points as plotted from 6 to 7½ years were as nearly as possible vertical therefore the beginning of the diminution of delivery due to the age of th pipe should be taken as beginning at 8 years—which was the period whic had elapsed when he had returned for the laying of the second line of pipe on the Thirlmere aqueduct, and had found the first pipe in a remarkabl good state of preservation. The "ageing" curve would then agree mor closely with the state of the pipe-line referred to by the late Mr. F. W Macaulay in his remarks in the discussion on Mr. Barnes's Paper.||

After the 20 years' period shown upon that curve, the rate of th reduction in the delivery would rapidly decrease, and in all probabilit would ultimately cease altogether long before the lapse of 100 years in th life of the pipes.

Dr. F. V. A. E. Engel observed that the Authors applied some of th latest results from Goettingen to a practical problem of hydraulic engineer ing. It seemed to be doubtful, however, whether those investigation were as yet sufficiently well-known to justify the somewhat too brief wa in which the various results were derived. The two references given o p. 100 §, in spite of being excellent Papers, could not be regarded as ful enough to be of assistance to the reader in following the Authors in thei present Paper. The engineer was inclined to check theoretical investiga tions by referring to actual test results. In that connexion, referenc could be made to Papers by J. Nikuradse * and H. Schlichting.†

Equation (4), p. 103 §, was only valid above a certain limiting Reynold number, a fact which was only mentioned two pages after the presentation of that equation. Instead of relationships (6A) ‡ and (6B), which were somewhat cumbersome to evaluate, it might be useful if the Authors were to give a few numerical values of the usual form of Reynolds numbe for various relative roughnesses.

In Dr. Engel's opinion there was one point in particular which deserve

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|| Footnote (*), p. 381.

§ Page numbers so marked refer to the Paper. (Journal Inst. C.E., vol. 7 (1937–38 (November, 1937).)—Sec. Inst. C.E.

* "Stroemungsgesetze in rauhen Rohren." V.D.I.-Forschungsheft 361, Berlin 1933.

† "Experimentelle Untersuchungen zum Rauhigkeitsproblem." *Ingenieur-Archiv.*, vol. 7 (1936), p. 1.

‡ Attention is drawn to the Corrigenda inserted at p. 1 of this volume of the Institution Journal (June 1938 issue).—Sec. Inst. C.E.

more elucidation; namely, the reference to the "constant" y_1. Prandtl and Schlichting gave 30 for the numerical value in equation (3), p. 103 §. Nikuradse's experiments gave the value of 29·3 on the basis of velocity-distribution measurements, and a value of about 30·1 for resistance-measurements. The Authors referred to values of 20 and 25, and used, for some reason, 33 in their equation (3). Those values seemed to indicate rather liberal limits for results obtained by applying equation (4). Was it correct that those comparatively large differences could be explained merely by the variation in the uniformity of the sand-layer? It might be of general interest if the Authors gave a more complete explanation of the constant y_1, and also indicated the various ways in which its value was determined.

Mr. E. H. Essex observed that the Authors had gone over much ground which had been previously discussed by mathematicians with no new development. The conclusion reached was that the rational coefficient (C in Chezy's formula) was again being accepted, and, as shown by the Authors, was represented in the customary German formulas by

$$\frac{C}{\sqrt{8\rho g}} = \frac{V}{\sqrt{8RSw}}.$$

On p. 103 § the Authors concluded their argument with the statement, "In any case, both theory and experiment show that y_1 is independent of the size of the pipe, so a single measurement of the resistance of one particular pipe can be used to predict the resistance of pipes of other sizes provided that all have identical surfaces." Mr. Essex was in complete agreement with that statement, and it had also been the considered opinion of the late Sir Richard Glazebrook, Hon. M. Inst. C.E., in his James Forrest Lecture,* who said that it was amply verified by the records of the Engineering Department of the National Physical Laboratory, as shown by the late Sir Thomas Stanton upon a curve of $R/\rho V^2$ values plotted against the Reynolds number. Sir Richard commented thereon, as follows: "the right-hand branch shows a value of $R/\rho V^2$ gradually decreasing as VD increases and tending to become a straight line parallel to the axis of abscissæ." Was he quite correct, however, in that conclusion? The Authors thought that he was so far as the frictional coefficient lay upon that one curve irrespective of the diameter of the pipe. Professor A. H. Gibson † and Mr. Frank Heywood † had, however, disagreed, and had attempted to show that $R/\rho V^2$ had to have a separate curve for each diameter, and they had further attempted to establish the fact that each

§ *Ibid.*

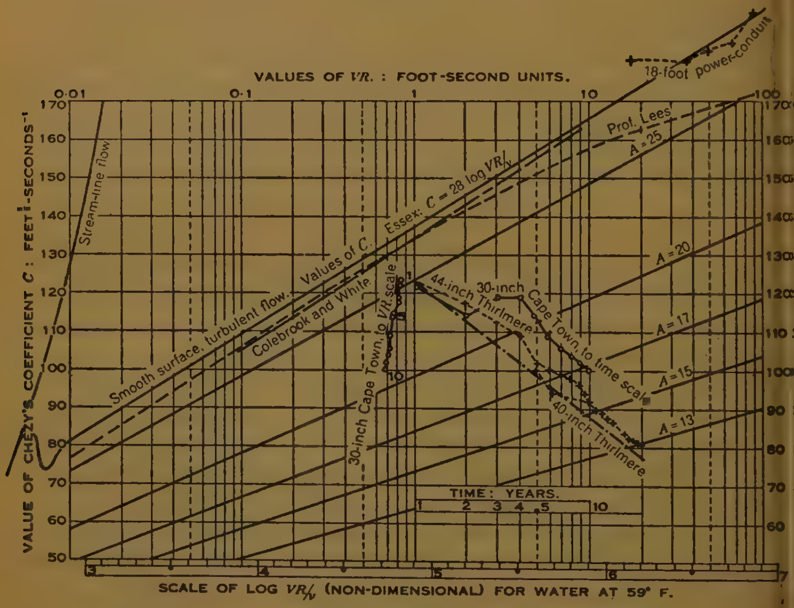
* "The Interdependence of Abstract Science and Engineering." Minutes of Proceedings Inst. C.E., vol. ccxvi (1922-23, Part II), p. 466.

† A. H. Gibson, "The Investigation of the Surge-Tank Problem by Model Experiments"; F. Heywood, "The Flow of Water in Pipes and Channels." Minutes of Proceedings Inst. C.E., vol. ccxix (1924–25, Part I), pp. 161 and 174.

of those innumerable curves was bound to follow the line of Professo[r] C. H. Lees's ‡ evaluation of Sir Thomas Stanton's curve, which Professo[r] Lees had evaluated as $R/\rho V^2 = A(^v/VD)^n + B$. That argument, [if] accepted, would lead to great confusion again, just when a clear con[-]ception was being obtained of the problem brought about by th[e] unlimited production of various new exponential formulas.

Mr. Essex was in entire disagreement with Sir Richard's concludin[g] statement, and he felt that it was a point of view which had at all times le[d] mathematicians astray; he thought that that was especially so in th[e]

Fig. 12.



case of Ganguillet, whose formula had done so much to detract from th[e] inestimable value and utility of Kutter's abstract science. The Authors in *Fig. 2* (p. 107 §) gave a curve for the German expression in the well[-]known pipe-resistance formula $h = \dfrac{\lambda \rho V^2}{2gd}$, or $\lambda = 8g/C^2$.

In *Fig. 12* Mr. Essex had transposed the Authors' line for λ in smoot[h] pipes plotted to arithmetical ordinates to give a value of C in foot-pound second units, and logarithmic abscissa of VR or VR/ν, together wit[h]

‡ "On the Flow of Viscous Fluids through Smooth Circular Pipes." Proc. Roy[.] Soc. (A), vol. xci (1914–15), p. 46.

§ *Ibid.*

rofessor Lees's evaluation of the Stanton curve and his own evaluation f that curve, which was $C = 28$ Log VR/ν; making no attempt to valuate the Authors' curve, he would like to ask them, firstly, did they onsider that their curve accurately represented Continental observations? econdly, was their curve not at variance with the contention of Sir ichard Glazebrook and many others that the curve would gradually ecome parallel to the abscissae, in the manner indicated in exaggerated orm by Professor Lees's curve, so that his curve failed to connect with he values of C obtained in the records of the Ontario power-conduit? hirdly, did they not think that the curve represented by the straight ne $C = 28 \log VR/\nu$ following the law of organic growth, was for all ractical purposes the most rational evaluation of the Stanton curve for mooth pipes?

The Authors then stated, on p. 105 §, that it remained only to discuss ow y_1, the distance from the wall where the velocity was zero, depended pon k, the size of the protuberance on the wall; Mr. Essex felt, however, hat to continue that argument was academic rather than practical, for f the value of Chezy's coefficient C were known in terms of $A \log VR/\nu$, the ugosity-ratio would be represented by the variation in the value of A, nd would be more assuredly arrived at from observations in the field han from experiments in the laboratory. Moreover, the application of he theory to the incrustation of cast-iron pipes became of less importance lue to the increasing use of pipes lined with cement or bitumen, and ractically all unlined pipes should be scraped at the end of 10 years.

Mr. Essex had shown * from a careful examination of recorded observa-ions in cast-iron, steel, concrete, and timber pipes under pressure that a alue of $C = 25 \log VR/\nu$ should be obtainable when the pipes were new with the exercise of reasonable care in construction, and he had shown hat line in *Fig. 12*, together with the recorded findings in the 30-inch ast-iron water-main supplying Capetown, recorded by the late Mr. D. E. Lloyd-Davies † from which record he had worked out the figures hown in Table II (p. 394).

It would be seen that the value of A in the formula $C = A \log VR/\nu$ had been 25·6 when the pipe was new, and that it had fallen to 21·1 in the 10th year, so that a fall in the value of A of 0·46 per annum represented the reduction in the capacity. By using the scale of years in *Fig. 12* and drawing the dotted line from the value of $C = 24$ found at the end of the first year, reciprocal to the line $24 \log VR/\nu$, a line was obtained

§ *Ibid.*

* "Abstract Science and Academic Theory in Relation to Hydraulic Flow in Concrete Pipes and Culverts." Proc. Inst. Mun. and County E., vol. lviii (1931–32), p. 1863.

† "The Works of the Augmentation of the Supply of Water to the City of Cape-town, South Africa." Minutes of Proceedings Inst. C.E., vol. 234 (1931–32, Part 2), p. 4.

TABLE II.

Age: years.	Q, discharge per 24 hours: million gallons.	$VR = Q/4{\cdot}25$.	$\log VR/\nu = \log VR + 4{\cdot}91$.	$C = VR \times 148$.	$A = C/\log (VR/\nu)$.	Remarks.
New	3·55	0·835	4·832	123·5	25·6	Diameter of pi[pe] = 30 inches. Perimeter = 7·[…] feet.
1	3·50	0·824	4·826	122	25·3	$R = 0{\cdot}625$ foot
2	3·28	0·772	4·798	114·2	23·9	$S = 0{\cdot}000187$
3	3·41	0·802	4·814	118·8	24·7	$\log 1/\nu = 4{\cdot}91$ for water
4	3·42	0·805	4·816	119·2	24·8	59° F.
5	3·27	0·770	4·796	114	23·8	$\frac{1}{\sqrt{R^3S}} = 148.$
6	3·12	0·735	4·776	108·9	22·8	
7	3·04	0·715	4·764	105·9	22·3	
8	3·02	0·7	4·761	103·6	21·8	
9	2·93	0·690	4·749	102·2	21·5	
10	2·87	0·675	4·739	100	21·1	

corresponding to that shown in *Fig. 4* (p. 110 §). Against that line M[r.] Essex had plotted the values of C to the time-scale of Mr. Lloyd-Davies[’s] observations, and also the Authors’ values of C for the 44-inch and 40-inc[h] Thirlmere pipes, taken from *Fig. 4* and based by the Authors upon a[n] assumed velocity of 4 feet per second. An allowance of 0·5 reduction pe[r] annum in the initial value of A for the calculation of the Chezy coefficien[t] C seemed to be a practical allowance to make for depreciation against tim[e] especially considering that the smaller diameters of 6 inches and unde[r] had frequently been laid to give a low initial efficiency of $C = 20$ lo[g] VR/ν owing to defective alignment at the joints; it would be of intere[st] to know what the Authors suggested as the correct allowance in term[s] of the Chezy coefficient C to be made in the ratio y_1/k, where k (th[e] protuberance) was on one side of the pipe only, with a correspondin[g] depression upon the opposite wall of the pipe.

The Authors, in reply, emphasized that in their Paper they quote[d] the Thirlmere experiments in support of, and not as basis for, their theory in no way was it based on them, as Mr. Barnett had erroneously concluded On the contrary, when the Paper had been first drafted the Authors ha[d] not considered those particular experiments, and on subsequentl[y] analysing them they had found that they were well worth inclusion a[s] they were so free from the usual experimental scatter. Mr. Barnett’[s] criticisms seemed well justified in the light of the present theory, but i[t] had to be remembered that the Thirlmere Paper * had been written som[e] 20 years ago when the causes of hydraulic resistance were not nearly s[o] well understood as at present, and it was hardly fair to Mr. Barnes t[o]

§ *Ibid.*

* Footnote (*), p. 381.

point out errors now. The present Authors quoted only the experimental results, and they were in no way biassed by the opinions, erroneous or otherwise, expressed in the early Paper.

Mr. Barnett was correct in thinking that the 44-inch pipe should deliver more than 28·6 per cent. of its calculated capacity at the end of 100 years; the present theory predicted 48 per cent. He was wrong, however, in suggesting that a deterioration of 13 per cent. in the first year or so must be interpreted as a diminution of diameter from 44 to 42 inches. That question was dealt with at some length on p. 104 §, where numerical examples were given based on the present theory, which in a simple and rational way showed that nodules only $\frac{1}{50}$ inch in height provided the explanation. Incredible as that might seem, the fact remained that it was on a sound theoretical basis well supported by experiment, in the light of which Mr. Barnett's valuable eye-witness description of the surfaces was seen to be in complete harmony with observed and with predicted diminution of flow.

Experiments in the Hydraulics Laboratory of the Civil Engineering Department of the Imperial College of Science and Technology, particulars of which would be published shortly, showed that the interesting type of asymmetrical irregularity described by Mr. Essex would have to be exaggerated until the lumps and hollows almost interlocked before one side visibly reacted upon the other to cause the central streamline (time-average) to become sinuous.

The Authors thanked Mr. Essex for drawing their attention to the Capetown measurements. On analysis those seemed consistent with the present theory, and had an initial time-shift of 2½ years, and an average growth-rate of $\alpha = 0{\cdot}007$ inch per year, a somewhat low value which might usefully be added to Table I (pp. 112 § and 113 §) by those who had the necessary further data concerning the Capetown water.

The opinions of earlier workers were always difficult to discuss after the lapse of many years. Glazebrook, for example, although speaking only 16 years ago, had been actually quoting opinions of 10 years earlier or more, and at that time no one had suspected that Stanton's and Pannel's experimental results were subject to the influence of roughness at the higher Reynolds numbers, nor that later experiments would show that the curious form of plotting used by them was also somewhat misleading. Professor Lees's formula, quoted by Mr. Essex, was essentially empirical, and did not pretend to cover the case of rough surfaces; published in 1914, it failed to agree with more extended experiments even for smooth surfaces, and so fell into disuse some 10 years later. Mr. Essex's own formula for smooth surfaces was a good approximation to the Karman-Prandtl formula so far as overall resistance was concerned, but it was not based on the internal motions which were the real cause of the resistance. His formulas

§ *Ibid.*

for rough surfaces, however, were widely at variance with the facts: a glance would show how different was the appearance of Mr. Essex's hypothetical *Fig. 12* (p. 392), and the Authors' experimental *Fig.* (p. 107 §). If Mr. Essex were to compare his formula with Nikuradse's laboratory experiments the discrepancy would be even greater.

Mr. Essex also proposed another ageing formula (p. 393) as an alternative to that of the Authors. The symbol A used by Mr. Essex denoted a different quantity from the Authors' symbol A, and α was also used differently, as the Authors had separated diameter, speed, and viscosity from both quantities. As a result they had only to tabulate some ten different values for different waters. Mr. Essex, with four times as many variables, would have to tabulate 10^4 similar values of his A to cover an equally wide range, and apart from that his formula was not as simple as the Authors'. It would be clear which treatment was the more practical.

Dr. Engel had noted that the 29·3 and the 30 of Nikuradse and of Prandtl differed from the 33 used by the present Authors for correlating y_1 with k. That was true; it was due to the use of limits in the integration (p. 102 §) which focussed attention upon the meaning of that constant and also to the more concise algebra which enabled the constant to be left undetermined until the end. There was then a choice of method, although both required that the velocity be known at some known distance from the wall. The Authors, on analysing Nikuradse's original data, found that according to his pitot-tube measurements, the ratio k/y_1 varied from 20 to 50, with an average of about 30, but with a wide range of uncertainty. The other method took advantage of equation (1B), p. 102 §, which showed that the mean velocity as determined by volumetric measurement of the rate of flow could be used since it occurred at the known distance 0·225R from the wall. On that basis, and using Nikuradse's mean-velocity values, the scatter was quite small and left but little doubt that the value of k/y_1 was very close to 33.

Prandtl used the value 30, apparently without noticing that the 1·74 he used elsewhere in the formula $\gamma/\sqrt{\lambda} = 2 \log r/k + 1{\cdot}74$ implied the use of the value 33; possibly the many algebraic steps he interposed between the two values obscured the discrepancy, or possibly he thought it too trivial for discussion.

From the practical point of view, the whole question might be likened to a book-keeping transaction. The values of k as tabulated for use by engineers had been obtained indirectly from measurements of hydraulic drag, and not directly from the physical size of the lumps. So long as the formula used with the Tables was that on which they were built then the ratio of k to y_1 seemed unimportant, although there were reasons which prevented its entire elimination, as desired by Mr. Essex. The value 33 enabled roughness to be expressed in terms of the well-known sand-scale

§ *Ibid.*

of Nikuradse, and so provided a simple and convenient mental picture; it also served as a basis for estimating hydraulic drag directly from the appearance of any particular surface, although quite crude approximations sufficed for both those purposes. Another reason for the need of the value 33 might be illustrated by reference to the valuable work of Schlichting, who, when seeking data for the drag of the riveted hulls of ships, and for the drag of riveted pipes, made experiments with a riveted plate forming the bottom of a wide flat channel. His values of k, obtained in that way, would not, however, be universally applicable to the three cases unless some factor akin to the value 33 were used in connexion with them. The value 1·74 of Prandtl given in the formula above was the logarithm of the square of 7·42, and that was the product of 0·225 (location of mean velocity) and 33 (wall-condition): it was only that second factor of the 7·42 which could be expected to apply to cases so different as the three that Schlichting considered. If the figure 7·42 were broken up into wrong factors such as 0·3 and 25, then the latter value was not independent of the location of mean velocity, and so, strictly speaking, applied only to flow in which the main body of fluid moved like that in the experiment. However, the logarithm helped to mask errors, and the final result was very much closer than many engineering calculations which no one thought of questioning. An error of 10 per cent. in k caused an error of only 1·2 foot$^{\frac{1}{2}}$-second^{-1} unit in Chezy's coefficient.

Dr. Engel asked for equation (6) in the form of Reynolds numbers versus roughness-ratios. Table III would assist comparison with values already familiar, although it was a more complicated system to use.

TABLE III.—RANGE OF REYNOLDS NUMBERS WITHIN WHICH ROUGHNESS AND VISCOSITY CAN INFLUENCE HYDRAULIC RESISTANCE.

Roughness ratio * d/k.	Lowest Reynolds number at which roughness has influence.	Highest Reynolds number at which viscosity still has importance.	Highest Reynolds number at which viscosity still has detectable influence.
30	2,000	10,000	21,000
100	8,000	45,000	90,000
300	27,000	150,000	310,000
1,000	110,000	600,000	1,200,000
3,000	360,000	2,000,000	4,100,000
10,000	1,300,000	8,000,000	16,000,000
30,000	4,400,000	26,000,000	52,000,000

The Authors thanked Dr. Engel for emphasizing that there were ranges

* For second column k denotes the size of the largest lumps; k denotes the mean size for the other columns.

of flow to which equation (4), p. 103 §, did not apply; they had hoped that the phrase "a particular case," which immediately preceeded the equation would convey the necessary warning, which might be repeated in another way by stating that, for practical purposes where it would be academic to quibble regarding the exact location of the point where two laws merged together more or less asymptotically, the central column of Table III gave the Reynolds numbers below which the present theory became unreliable.

They wished to draw attention to an error † in equation (6A), p. 105 §, whilst in *Fig. 2* (p. 107 §) the code following each curve concerned the lining and metal, the diameter in inches, and the first letter of the experimenter's surname. The smooth-pipe curve was plotted according to the formula $C = \sqrt{32g} \log \frac{1\cdot13 d\sqrt{\tau/\rho}}{\nu}$. In *Fig. 4* (p. 110 §) both curves were theoretical, yet they were seen to be quite as close to the points as those of *Fig. 11* (p. 389) * in which the curves had been shaped by eye to give the best possible fit, whereas the theory was built without reference to the Thirlmere data. The choice of variable in *Fig. 11* seemed unsatisfactory, since an error in estimation of the initial capacity of the pipe was bound to throw out all the later values by a proportionate amount, whereas the method of plotting given by the Authors in *Fig. 4* (p. 110 §) was free from that defect; in fact, the idea that initial capacity was unimportant was one of the central features of their theory. The changes were rapid at first, even though much of the initial surface of the pipe might remain visible: experiments by the Authors, referred to on p. 103 §, showed that in an extreme case 95 per cent. of the area of the original surface might remain visible yet contribute nothing to the total drag. Hence *Fig. 11* could be quite misleading. Neither *Fig. 11* nor *Fig. 4* was valid for other sizes of pipe or for other waters; those variables were included in the Authors' general solution as represented by equations (11) or (11A) (p. 116 §).

The use by the Authors of the word "approximate" in connexion with equation (11A) had been misinterpreted; the expression was actually correct to the order of one part in a thousand over the whole range. Still, in the strict algebraic sense, equations (11) and (11A) were not identical, so that there was always the possibility that someone, unaware of the approximation, might attempt to reason somewhat on the following lines. The identity 1 = 1, on substituting the approximation 1·001 for 1, became 1·001 = 1, which by subtracting 1 from both sides and multiplying by 1,000, yielded the absurdity 1 = 0! On p. 116 § the word "approximate"

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§ *Ibid.*

† Indicated in a corrigendum facing p. 1 of the Institution Journal for June 1938 (vol. 9 (1937–38).)—Sec. Inst. C.E.

* The abscissa and ordinate of *Fig. 11* seemed to be interchanged in contravention of the rule that the dependent variable should appear as ordinate. On turning *Fig. 11* on its side it might be compared with *Fig. 4*.—C.F.C. and C.M.W.

just before equation (11A) should be deleted, and should also be deleted from the last sentence of the introduction (p. 99 §).

Mr. Essex in his opening sentence fell into two errors. He was entirely wrong in thinking that the identity $C/\sqrt{8\rho g} = V/\sqrt{8RSw}$ was part of, or was even discussed in, the Paper, and he was equally in error in thinking that the Authors were following earlier work without contributing new matter. So far as they knew, of the eighteen numbered equations in the Paper, only two had been published previously, though another two had appeared in a different form. Even the three pages which appeared to summarize earlier work actually contained new matter, such as the use of limits (top of p. 101 §); *Fig. 1* (p. 101 §), showing the intersection of all curves at mean velocity, together with the two extreme curves; the integration (top of p. 102 §); equation (1B) showing the location of the filament of mean velocity; and equation (5) showing that roughness was more important than loss of area, and other novel points. The remainder of the Paper was new not only in statement but in its whole outlook, for it was based on the idea that a designer should think perhaps 20 years ahead, and should design on the basis of conditions then, instead of trying, as at present, to apply corrections of uncertain amount to an incorrect design based on transitory newness which might last for a few weeks only, or indeed which might have been lost before the pipes were laid. It would be as unreasonable to attempt to forecast the appearance of a painting from a description of the surface of the canvas before the artist had touched it with his brush, as to attempt to predict the ultimate surface of a pipe on the basis of its initial polish. There might be exceptions, but in general, after a lapse of years all degrees of roughness were found in all types of pipes, and the initial and final forms of the surfaces were independent of each other, or nearly so. The Authors thought that in that respect the whole conception was novel in relegating the new condition to the very subsidiary position in which it was regarded as an unimportant factor, to be treated by adding a correction (usually about 6 months) to the recorded age of the pipe.

Hence, regarding the Paper as a whole, the Authors were confident in their view that it was new not only in statement but in subject-matter, at least so far as the engineering part was concerned. The rest of it contained new detail as well as a restatement of recently established facts as yet unknown to most engineers; in fact, Dr. Engel on that very point took the Authors to task for having unduly compressed unfamiliar matter. Further references to published work on the subject would be found in another Paper [1] by the Authors.

§ *Ibid.*
[1] "Experiments with Fluid Friction in Roughened Pipes." Proc. Roy. Soc. (A), vol. 161 (1937), p. 367.

Paper No. 5080.

"The Failure of Girders Under Repeated Stresses." †

By Professor Frederick Charles Lea, O.B.E., D.Sc. (Eng.), M. Inst. C.E., and John Gwynne Whitman, M. Eng.

Correspondence.

Mr. W. O. Leitch observed that in the tests of girders with rivet-holes breaks occurred under a range of stress of from 11·1 to 10·25 tons per square inch. Whilst new girders, including impact-allowances, were not designed to bear such high stresses, old girders, before being condemned because of increase of loading after construction, probably bore stresses approaching those figures. It would, therefore, be very interesting to make further tests under bridge-conditions; heavy trains of, say, one hundred axles at intervals of 3 minutes would represent a very busy railway, and consequently one hundred repetitions of stress, followed by a 3-minute rest, and so on, would be nearer actual bridge-conditions. In the floor-system the range might be from nil to, say, 10 tons per square inch, depending on the space between the axles of the trains, but for the main girders the stress, except in short spans, would gradually increase to 10 tons per square inch, would then vary, say, from 7 to 10 tons per square inch with the variation of impact, and would then decrease to nil, a much less severe condition than continuous repetitions of from nil to a stress of 10 tons per square inch.

The Authors, in reply, agreed that stresses comparable to those at which the tested girders broke were possible under working conditions, in very special cases. The fractures in the test-specimens only occurred at those stresses after more than 5×10^6 repetitions, and there was no reason to think that the rate at which axle-loads would be applied would very seriously lower the fatigue-range. The only serious danger would be if, due to some abnormal condition, a very high local stress were produced which started even a very small crack. The application of the slowly-applied axle-loads might then, at less stress than that which produced the first crack, cause a progression of the crack. There seemed to be no danger to riveted girders due to the application of rolling loads, repeated for very many years, provided that the stress did not exceed 10 tons per square inch. If the actual stress produced were less than that there was a real factor of safety greater than unity.

† Journal Inst. C.E., vol. 7 (1937–38), p. 119 (November 1937).

CORRESPONDENCE
ON PAPERS PUBLISHED IN
DECEMBER 1937 JOURNAL.

Paper No. 5148.

"Combustion-Efficiencies of Gas and Oil Engines." †

By William Alfred Tookey, M. Inst. C.E.

Correspondence.

Captain R. W. A. Brewer observed that the Paper was practically confined to a consideration of engines of the four-stroke-cycle type, and that the statement was made that no advance had been made in combustion-efficiency since the Reports of Professors Bertram Hopkinson and F. W. Burstall in 1908; *Fig. 6* (p. 181 §) was given as a proof that that statement was correct. The improvement in two-stroke-cycle engines had, however, been quite marked, and his Paper read before the Society of Engineers,* and discussed by Mr. Tookey, gave data on some developments in the United States under his direction. His results fell in the general group of points shown in *Fig. 6* for an engine-speed of 1,200 revolutions per minute, showing that the modern two-stroke-cycle commercial type of high-speed gas engine was comparable with the four-stroke engines referred to by the Author. Further information would be found in Captain Brewer's Paper.* In single-cylinder two-stroke test engines, better results had been attained, and indicator mean pressures of between 100 and 108 lb. per square inch had been recorded for mixture-strengths of between 35 and 39 B.Th.U. per cubic foot of total cylinder volume. An eight-cylinder two-stroke radial power-unit of the type described in his Paper * naturally developed less specific power, and its general performance was not so good, but it was well worth further development. Since then other engines had been made to his designs with cylinders having a bore and stroke of 8 inches. Curves relating mean indicated pressure to mixture-strength per cubic foot of total cylinder volume showed that the results from four-stroke-cycle and from two-stroke-cycle engines were in general agreement.

† Journal Inst. C.E., vol. 7 (1937–38), p. 166 (December 1937).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

* "The Development of a Two-Stroke Cycle Gas Engine." Trans. Soc. E., 1934, p. 90.

In the Brewer engine, the cylinder was filled with fresh air at pressure of approximately 2·5 lb. per square inch at the instant of port closing, and gas was then injected under a pressure of up to 40 lb. per square inch. It was thought that a method of computation based on the actual volumes of gas and air supplied prior to combustion should have been adopted; the Author had pointed out, however, that his method was more satisfactory in showing a true comparison between various trials, and that had proved to be the case.

Mr. G. H. Paulin, referring to p. 167 §, offered a more satisfactory explanation of the exhaust-scavenging process. When the exhaust-valve opened there was a discharge of exhaust-products at comparatively high pressure into the exhaust-pipe. That caused a wave of pressure which travelled towards the silencer end of the pipe with the same speed as sound would travel in an atmosphere similar to that in the pipe. Due to the elasticity and inertia of the atmosphere in the exhaust-pipe, that initial wave of pressure was followed by a rarefaction and again by pressure-wave, and so on. The waves of pressure and rarefaction travelled on, until at the silencer end of the pipe they were reflected successively and travelled back to the engine, where they were again reflected. Thus there was set up in the exhaust-pipe a series of waves of pressure and rarefaction which travelled back and forth. If the pipe were made of such a length that a wave of rarefaction arrived at the exhaust-valve when it was on the point of closing, the pressure, and thus the weight of the residual gas in the clearance space, would be a minimum. It would be seen that there was, however, a possibility of there being a pressure-wave at the exhaust-valve at the instant of its closure, depending on the length of the exhaust-pipe and on the speed of the engine, so that for most efficient scavenging it would be necessary to "tune" the exhaust-pipe length to the engine-speed, and not just to make it 12 or more feet long.

The values of P_m in Table III (p. 171 §) for throttle-governed engines did not altogether agree with *Fig. 1* or Table I (p. 169 §). On bringing them into agreement, as shown in Table XVI, more grounds were found

TABLE XVI.

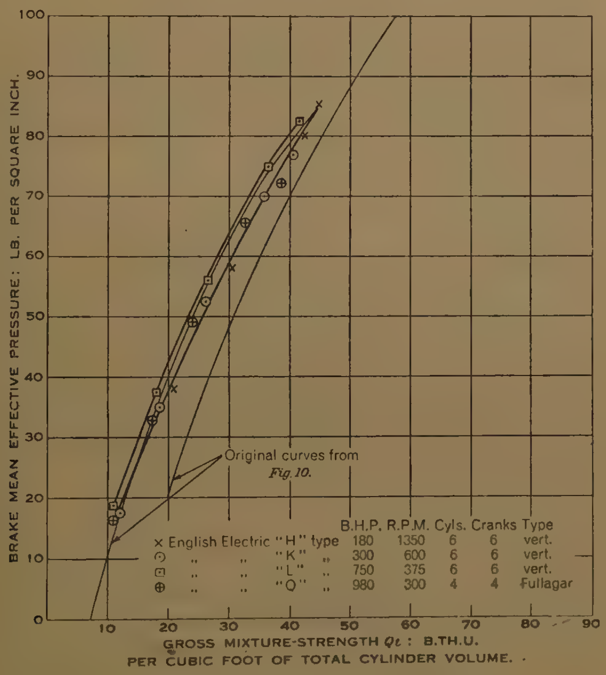
Q_t: B.Th.U. per cubic foot.	Indicator mean pressure, P_m: lb. per square inch.	Difference-factor.	Hit-and-miss governing difference-factor.
20–25	42·5–52·5	2	—
25–30	52·5–62·5	2	—
30–35	62·5–70·5	1·6	—
35–40	70·5–78	1·5	1·6
40–45	78 –85	1·4	1·5
45–50	85 –89	0·8	1·4

§ *Ibid.*

or the remark in the first paragraph on p. 171 § to the effect that the hit-and-miss governed engine was more efficient in converting heat into work than the throttle-governed engine over the range of mixture-strengths from 35 B.Th.U. per cubic foot to 50 B.Th.U. per cubic foot.

To prove that the high-speed engine need not be at so great a disadvantage as *Fig. 10* (p. 185 §) showed, the curves in *Fig. 10* had been replotted in *Fig. 25*, and the particulars of a number of English Electric engines had been added. All those engines were of the direct-injection type. Table XI (p. 186 §) had been extended, as shown in Table XVII

Fig. 25.



(p. 404), over the portion common to the industrial and high-speed types of engines, and to include the Tookey factors.

The values of n–1 in Table XIV (p. 189 §) which were based on *Fig. 11* (p. 187 §), were incorrect, because n–1 could only be based on indicated pressures, and not on brake pressures; further, it was thought that the value of n–1 could not be evaluated from the "difference of T_m per B.Th.U. per cubic foot of swept and clearance volumes." It could be calculated from the T_m-value, but surely it could not be obtained from

§ *Ibid.*

TABLE XVII.

Q_t : B.Th.U. per cubic foot.	P_n : lb. per square inch (industrial type).	P_n : lb. per square inch (high-speed type).	T_n (industrial type).	T_n (high-speed i type—Paxman-Ricardo).
20	39	23	1·95	1·15
25	51	36	2·04	1·44
30	62	48	2·07	1·6
35	72	60	2·06	1·71
40	82	70	2·05	1·75

the $\triangle T_m$-values, where $\triangle T_m$ denoted the change in value of T_m for uni change in B.Th.U. per cubic foot of swept and clearance volumes ?

The Author, in reply, observed that the further confirmation that Mr Brewer had adduced of combustion-efficiency being independent of th actual cycle of operations was very welcome. Mr. Brewer's engine, a described in his Paper †, possessed many novel features, and was designe primarily to use high-pressure natural gas which could be injected into a already assembled air-charge. In the Erren engine also, compressed coa gas was injected after the completion of the charging period, whether o either the two- or the four-stroke cycle, and in that respect it was simila Mr. Brewer's confirmation that truer comparisons were obtained on th basis of total cylinder volume than on piston-displacement volume was als valuable. With regard to the exhaust scavenging process, the Autho agreed that Mr. Paulin's more lengthy explanation had merits over hi own condensed phrases, but it was thought to be unnecessary to reiterat a generally-accepted statement as to pressure-waves in an exhaust-pip system.

Mr. Paulin had evidently failed to realize that once the engine friction mean pressures had been taken into account in $P_m - P_f = P_n$, the value of $\triangle T_m$ and of $\triangle T_n$ were bound to be one and the same. It was surel well worth while drawing attention to the changing values of $\triangle T_m$ or $\triangle T_n$ with mixture-strength variations. It might be that the evaluatio of $n - 1$ from $\triangle T_m$ or $\triangle T_n$ was unorthodox, but if excuse be neede material progress had often resulted from such departures. In the preser instance the whole tenor of the Paper was to co-ordinate practice wit theory, and the mathematical evaluation of $n - 1$ was deemed by th Author to be sufficiently interesting to warrant its inclusion in Table XI (p. 189 §), as with Table XV, p. 190 § (calculated on a different basis), was possible to realize the gradual deterioration of performance with in crease of loading, whether by increment as in Table XIV or as a who in Table XV, and by hypothesis to indicate apparent specific-heat ratic at the higher temperatures.

† Footnote (*), p. 401. § *Ibid.*

Paper No. 5146.

"Dover Train-Ferry Dock."†

By George Ellson, O.B.E., M. Inst. C.E.

Correspondence.

Mr. W. O. Leitch observed that the train-ferry across the Yangtse-Kiang at Nanking had been referred to, with particular reference to the apron; a movement of 16 inches on each side of the horizontal was available owing to the flexibility of the apron-bracing, without any adverse results. He might mention that a complete description of that ferry and all the works at Nanking had been published elsewhere.*

The plan adopted at Dover was a very interesting example of the method of overcoming the difficulties arising from a stormy sea and a restricted land-site. In considering whether such a plan might be adopted at other places, it would be interesting to know the time taken to turn the ferry-steamer around, for it to enter the dock, and to set the ferry at the correct level ready to attach the apron.

Apart from the difficulties arising from fissures in the chalk, it would seem that enclosing an area in sheet-piles did not offer much prospect of success, for an interlocking steel pile driven to a depth of 5 feet would act very like a rock-breaker and would leave a space between the pile and the unbroken chalk which would be difficult to seal under a head of 50 feet of water.

Mr. James Mitchell observed that the existence of large fissures in chalk, under a head of 50 feet of water, presented a serious problem which, as was mentioned in the Paper, was further complicated by the restrictions imposed on the blanketing of the surface, by mud or clay, within the harbour-area. The extremely low rate of output from the dredger was a testimony to the hardness of the chalk, but in view of the uncertainty regarding the size and extent of the fissures in it, the change in the general design of the method of construction of the works appeared to have been fully justified and quite successful. The caisson was a costly method of providing for the temporary closing of the dock-entrance, but it had the great advantage of being easy either to place or to remove as often as might be desired. Had it been designed so as to be utilized elsewhere? With reference to the grabbing of dredged material from barges and loading into railway wagons (p. 231 §), the method that had been

† Journal Inst. C.E., vol. 7 (1937–38), p. 223 (December 1937).

* A. E. Reid, "A Short Account of the Design and Construction of the Nanking-Pukow Train Ferry." Proc. Eng. Soc. China, vol. 32 (1933–34), p. 61.

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

adopted at Wick harbour-works, in the North of Scotland, under som what similar circumstances, might be mentioned. In that case, the bo portions of tipping-wagons had been lifted off their carriages and had bee laid on flat-decked rectangular punts, each punt carrying twelve boxe arranged in two rows. The punt was brought alongside the dredger, an the six boxes on one side were filled one at a time, after which the punt wa swung round and the remaining boxes filled. The punt was then haule alongside a crane, which lifted the boxes (previously fitted with speci lifting-chains) on to their carriages, ready to be hauled to the place c deposit. The courses of the blockwork-walls (Fig. 2, Plate 1 (facin p. 260 §)) were very efficiently designed to resist sliding, with the exceptio of the lowest course, as the small fillet of concrete shown—which wa presumably placed after the blocks had been laid—did not appear likel to have much value in that respect. With reference to the formation c laitance on the upper surface of concrete deposited by means of tremi pipes (pp. 231 § *et seq.*), it was not clear why that should have occurre unless it was due to churning of the concrete by the movements of th divers. Perhaps the Author would give his views on the matter. Con sidering the difficulty of producing sound work under such conditions, was rather surprising to find that the concrete deposited under water, b means of skips, between the girders of the pump-room floor (p. 231 § had proved to be so water-tight.

For the unusual and exacting requirements of the dock-gates ther were no other gates so well adapted as those of the Box single-leaf typ The ingenious modification of the gate-hinge, whereby the gate wa enabled to resist pressure from either side, the shortness of the gate-strut with the nearness to the meeting-faces of their point of application, an the conical wedges used for tightening them against the gates, were a admirable features, and were a tribute to the skill of Mr. Edward Bo M. Inst. C.E., and to his long and intimate experience of that class of work The complete formation of two gate-hinges under water was an entirel novel undertaking. Mr. Mitchell had had experience of the setting an fixing of such a gate-hinge, and of floating the gate into position within very confined space inside a cofferdam, and he could therefore appreciat to some extent the difficulty of the work at Dover, and the high degree o accuracy of the results obtained. The grillage-bearings, with their level ling-towers, and the arrangement of adjusting-screws that could b regulated by the divers, were especially worthy of note; so also was the us of the gates themselves in the formation of a cofferdam, to permit the gate hinges to be inspected after being set in their final position. The modifica tions of the dock-sill and the gate-keels for that purpose were simple an effective. No arrangement was shown for supporting the gates when i their fully-open position, but presumably that was done in the usual way

§ *Ibid.*

y means of pillars supported by the floor of the dock-entrance. Perhaps he Author would state how the friction-clutches of the gate-lowering gear vere adjusted so as just to yield with a load of 60 tons. Such clutches vere extensively used in dredging-gear, and considerable difficulty was ound both in adjusting them and in keeping them in adjustment. Perhaps he Author would also state the reason for having three different springs n the buffer shown in *Figs. 17* (p. 243 §). Were they intended to come nto action consecutively?

Mr. E. G. Walker observed that the difficulties of construction had een increased very greatly by the necessity of carrying out the whole of he work in the wet. The impression derived from reading the Paper vas that no undue troubles had been experienced in depositing the concrete nder water through submerged tremie pipes. The Author, however, n p. 232 §, dismissed the subject of the formation of laitance with the tatement that laitance had to be removed. In ordinary concreting perations in the dry, that was a normal and not particularly difficult peration, but under water the conditions were far less easy. Some escription of the method of cleaning employed, and of the results obtained, vould be of interest.

The Box dock-gate had been devised by its inventor, Mr. Edward Box, M. Inst. C.E., to meet the special requirements of the short graving docks n the river Tyne, where the land-space available was insufficient to permit nitering gates to be used, and sliding caissons were equally unsuitable, wing both to cost and to their occupation of valuable river-side frontage. An extensive foreshore, however, provided ample space for a gate hinged orizontally. The application of that gate at Dover was an extension of he original idea. The situation at Dover was very exposed, and the onditions of operation much more severe than in a river; presumably or those reasons it was deemed necessary to fit strut-gates and to duplicate he whole installation. Those additions had, however, offset entirely the dvantage of reduction in the length of the dock, which was a feature of he original application of the Box gate. It would be of considerable nterest to obtain the results of the working of the ferry-dock gates over a ong period.

No information was given with regard to the cost of the works. Some tatement of expenditure, with unit costs, suitably divided amongst the arious classes of work involved, would be of considerable value, having n mind the difficulties that had to be surmounted. Information relative o the increased cost which resulted from building under water, as compared vith the original intention of building inside a cofferdam, would also be seful.

The Author, in reply, observed that the usual procedure after a ferry-oat had entered Dover Harbour and turned, was for it to warp alongside

§ *Ibid.*

the jetty and to enter the ferry-dock stern first. The dock-gates wer then closed, the necessary pumping was carried out and the link-span wa locked in position on the vessel. The time absorbed by those operatio varied with the state of the tide. The majority of the timings were abo as follows: from ferry jetty to inside dock, 5 minutes; average furthe interval until link-span bridge was locked in position on vessel, 23 minute The lowest timing for those two operations under the most favourab conditions was about 14 minutes.

Regarding Mr. Leitch's remark that enclosing an area in sheet-pil did not offer much prospect of success, it should be pointed out that tha was not what had been originally proposed; the original scheme had bee as described in the sixth paragraph on p. 226 §, namely, a cofferdam wit two rows of steel sheet-piles spaced at suitable intervals by steel diaphragm and filled in between with suitable material. The distance apart of th two rows of steel sheet-piles was to have ranged from 30 feet to 25 fee Apart from the question of fissures in the sea-floor such a cofferdam coul no doubt have been properly sealed.

With regard to the temporary caisson which was used for closing th ferry-dock entrance, it was at one time thought that it might form part o the foundation of the jetty, but that was not the controlling consideratio regarding its adoption. Actually, its portability proved very valuab during the carrying out of the work, and it was being retained for use i possible emergency.

The small fillet of concrete to which Mr. Mitchell referred was con structed for sealing purposes, and such a fillet had been found effective i that respect in similar previous works. With reference to the formatio of laitance, that invariably occurred on the top surface of concrete whic was laid under water if the operations ceased while the top of such concret was covered with water. It was not considered that that was due to an churning of the concrete. The removal of laitance under water was no necessary on any occasion in the course of the work, but it was found tha the top of concrete which was finished in the tidal depth of the water whos surface was exposed at low tide invariably had laitance on the top, an such laitance was removed in the dry. In no case was the laying of under water concrete stopped at such a height as to render the formation c laitance possible.

As originally constructed the weight of the dock-gates when in thei fully-open position was taken on slightly projecting blocks of concret laid on the bottom of the dock-aprons through the gate-rests shown i Fig. 7, Plate 2 (facing p. 260 §). There was one gate-rest on the centr line of each dock-gate at the height shown. Since the dock had been i work, however, owing to the wear between the timber rest and the concret blocks, it had been found desirable to fix two gate-rests on each gat

§ *Ibid.*

Those rests were spaced at 42-foot centres, and each had double the bearing area of the original one; they were also fixed higher up the gate.

He wished to point out that a slight error had occurred in the Paper. On p. 238 §, lines 13 and 17, the words "band-brake" should be substituted for "friction-clutch."

Regarding the method of preventing the hoisting ropes from being loaded beyond 60 tons per rope, it might be added that the requisite tension inducing friction between the band and the drum on the main shaft was obtained by weights suspended at the end of lever-arms, and thus the pressure between the band and the drum could be calculated accurately. Actually, it was found by experiment that the final adjustment required to ensure that the 60-ton tension was not exceeded was very small.

The object in having the three different springs shown in *Figs. 17* (p. 243 §) was to ensure that the maximum amount of compressibility was obtained in the internal diameter of the buffer, and they all came into action simultaneously.

The reasons for the fitting of the strut-gates and their duplication were given in the first paragraph on p. 238 §, each gate being capable of maintaining a head of water either from the inside or from outside of the gate independently of the other.

The conditions at Dover were different from those described by Mr. E. G. Walker which existed on the river Tyne, to meet which the Box type of gate had been originally designed. Nevertheless, it was considered that that type of gate was particularly adapted to the conditions which had to be provided for at Dover, although the reduction in the length of the side walls referred to could not be obtained there as it was not permissible for the seas to strike the gates without the protection of side walls.

The cost of the dock itself, including the pontoon and the temporary caisson, but excluding the entrance-gates, was 3*s*. 5*d*. per foot cube of void contained in the dock when the length was measured from the face of the dock-heads to the centre of the inner rounded end of the dock.

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§ *Ibid.*

Paper No. 5079.

"The Effect of the Form of Cross-Section on the Capacity and Cost of Trunk Sewers." †

By Thomas Donkin, Assoc. M. Inst. C.E.

Correspondence.

Mr. E. H. Essex pointed out that the Author, in his opening statemen on p. 261 § that "When considering the design of a trunk sewer, the usua practice is to use either a circular or an egg-shaped form of cross-section" overlooked the fact that engineers formerly made use of other cross sections. Instances were to be found in the records of Kutter's Abstrac Science ‡, and the statement could certainly not be said to hold goo since May 1936, when Mr. J. L. Davies had designed a U-shaped trun sewer 1½ mile long *; he had submitted the design to the Ministr of Health for their approval, and he succeeded, after frequent reference to the book of Tables and diagrams referred to by the Author on p. 266 § in persuading the Ministry somewhat tardily to agree to the grantin of the necessary permission to raise a loan for what their official adviser thought fit to inform him was an entirely new departure in sewer-construc tion. The work had since been completed at Leatherhead by Mr. Davies the design showing many advantages both during construction and in afte use. There seemed to be great misconception of the very decided ad vantages of that U-shaped design for trunk sewers; for although a semi circular channel gave as good a cross section as any for the bottom hal of a sewer, and the diameter of the channel could be made to suit the available gradient to give a self-cleansing gravity flow for the minimum discharge, the full circular sewer was the worst possible cross section for efficiency of discharge, and were it not for its constructional advantages the circular section would, in all probability, have long since been abandoned.

On p. 262 § the Author stated that "When calculating the size of any particular type of sewer, a gradient is first of all assumed, from which the size is obtained." Surely that was as novel a procedure as the suggestion

† Journal Inst. C.E., vol. 7 (1937–38), p. 261 (December 1937.)

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

‡ W. R. Kutter, "Versuch zur Aufstellung einer neuen allgemeinen Formel für die gleichförmige Bewegung des Wassers in Canälen und Flüssen." Second edition, 1877. *Translated into English by* R. Hering and J. C. Trautwine *and published in 1899.*

* "Sewerage and Sewage Disposal in a Newly Constituted Urban Area." Journal Inst. M. & Cy. E., vol. lxiii (1936–37), p. 110.

to design the sewer to suit one particular formula? In nearly every instance the engineer found his gradient decided for him beforehand by the natural contour of the drainage-area. At Leatherhead it was desired to raise the discharge-level at the sewage-disposal works by at least 18 inches, in order to give better distribution over the filter-beds, and a gradient of 1 in 1,000 was all that was available. Any circular sewer should have a self-cleansing velocity of 1½ foot per second at the proportionate depth of 0·2, which represented 12 times the minimum dry-weather flow when full, and thus the ratio $V/\sqrt{S}$ had to equal the value of $C\sqrt{R}$. To have taken a value of $C{=}124\sqrt[6]{R}$ would have meant that the sewer would have needed to be designed to a size larger than that adopted by Mr. Davies.

On p. 275 § the Author stated, very erroneously in Mr. Essex's opinion, that "it is the crown-gradient and not the invert-gradient which decides the capacity of the sewer and provides the datum from which a basis of comparison can be made"; on p. 266 § he also made a statement which could not be supported by facts: "it is obvious that, for the same diameters, the capacity of the U-shaped section is 13·80 per cent. greater than the capacity of the circular section." Mr. Essex had prepared a Table (part of which was shown as Table XVI, pp. 412–413 §) and *Fig. 7* (p. 414 §) to show the discharge of Mr. Davies's U-shaped culvert as compared with the discharge of a 24-inch circular sewer of equal area; it would be seen that the maximum discharge of the U-shaped sewer was 10 cusecs, compared with 8·15 cusecs for circular section (or 12·25 per cent. more), when not quite full, and 8 cusecs, compared with 7·74 cusecs (showing an advantage of 10·3 per cent.) when flowing full, that was to say, under pressure.

The Author further stated on p. 269 § that the dry-weather flow in most trunk sewers forming part of a combined system represented something in the neighbourhood of 1 per cent. of the total discharge, and, whilst not accepting the accuracy of that statement, Mr. Essex would like to point out that it emphasized the advantage of the U-shaped cross section, which could be regulated to give a self-cleansing velocity for the dry-weather flow and provided ample top accommodation for storm-flows before relief overflows came into action. The late Sir Joseph Bazalgette, Past-President Inst. C.E., had said that "trunk sewers should never be called upon to discharge under pressure flow," and that statement was a strong argument in favour of the U-shaped cross section; with that section the sewer could not become surcharged so long as the outlet was free, whereas the upper half of the circular sewer was always more or less under pressure-flow, with the result that at every inspection-manhole the flow spread itself over the benchings and the outlet became surcharged. With the U-shaped section, however, the flow passed through the inspec-

§ *Ibid.*

TABL

Depth: feet.	Depth / diameter.	Area: square feet.	Perimeter: feet.	Hydraulic mean depth: foot.	Velocity, V: feet per second.
24-inch circular section: S=0·00077; fall=					
0·1	0·05	0·058	0·902	0·065	0·62
0·2	0·1	0·164	1·286	0·127	1·00
0·4	0·2	0·447	1·854	0·242	1·53
0·5	0·25	0·614	2·094	0·293	1·73
0·6	0·3	0·793	2·32	0·342	1·92
0·8	0·4	1·173	2·73	0·429	2·21
0·836	0·418	1·241	2·80	0·442	2·29
1·0	0·5	1·571	3·142	0·500	2·46
1·2	0·6	1·971	3·55	0·555	2·62
1·4	0·7	2·349	3·97	0·592	2·72
1·6	0·8	2·694	4·43	0·608	2·77
1·8	0·9	2·978	4·99	0·596	2·74
2·0	1·0	3·142	6·28	0·500	2·46
25½-inch × 21-inch U-shaped section: S=0·00092					
0·087	0·05	0·045	0·790	0·057	0·62
0·175	0·1	0·1245	1·127	0·1112	1·00
0·35	0·2	0·342	1·622	0·211	1·53
0·525	0·3	0·606	2·03	0·299	1·92
0·700	0·4	0·899	2·398	0·376	2·21
0·823	0·47	1·105	2·64	0·418	2·39
0·875	0·5	1·202	2·745	0·4375	2·46
1·0	—	1·423	3·00	0·474	2·62
1·2	—	1·782	3·40	0·524	2·80
1·4	—	2·149	3·80	0·566	2·92
1·6	—	2·524	4·20	0·602	3·02
1·8	—	2·907	4·60	0·632	3·13
1·92	—	3·1416	4·84	0·650	3·18
1·92	—	3·1416	6·84	0·460	2·55
2·0	—	3·298	5·00	0·659	3·22
2·125	—	3·546	5·25	0·675	3·27
2·125	—	3·546	7·25	0·490	2·64
3·125*	—	5·55	7·25	0·766	3·55

* After sewer-depth increased by 12 inches.

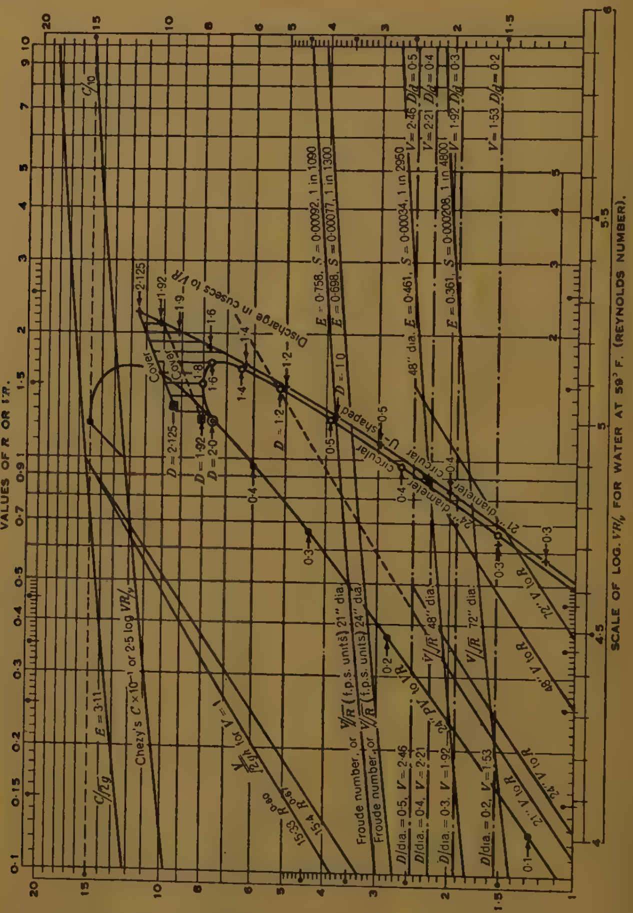
XVI.

Discharge, Q: cusecs.	VR.	$\log \frac{VR}{v}$	Chezy's C.	$V/\sqrt{R}$	$\frac{P}{R}$.
in 1,300 (if $C=124\sqrt[6]{R}$, gradient is 1 in 1,000).					
			$A=25$	$E=0{\cdot}698$	
0·035	0·041	3·52	88	2·44	13·81
0·164	0·127	4·02	100·5	2·81	10·11
0·684	0·370	4·48	112	3·11	7·69
1·06	0·507	4·61	115·2	3·20	7·15
1·52	0·656	4·72	118	3·28	6·77
2·60	0·947	4·89	122	3·38	6·37
2·80	1·00	4·91	122·7	3·40	6·35
3·87	1·23	5·00	125	3·47	6·28
5·16	1·45	5·06	126·5	3·52	6·40
6·39	1·61	5·11	127·7	3·54	6·70
7·41	1·68	5·13	128·2	3·56	7·27
8·15	1·63	5·12	128	3·55	9·43
7·74	1·23	5·00	125	3·47	12·57
fall 1 in 1,090 (if $C=124\sqrt[6]{R}$, gradient is 1 in 850).					
			$A=25$	$E=0{\cdot}758$	
0·0278	0·0353	3·46	86·5	2·60	13·81
0·1245	0·1112	3·96	99·0	3·00	10·11
0·524	0·323	4·42	110·5	3·33	7·69
1·162	0·573	4·67	116·7	3·51	6·77
1·985	0·828	4·83	120·7	3·60	6·37
2·64	1·00	4·91	122·7	3·71	6·30
2·96	1·075	4·94	123·5	3·72	6·28
3·73	1·24	5·00	125	3·80	6·34
5·00	1·47	5·08	127	3·87	6·50
6·27	1·65	5·13	128·2	3·88	6·70
7·63	1·82	5·17	129·2	3·92	7·00
9·11	1·98	5·20	130	3·94	7·30
10·0	2·07	5·22	130·5	3·96	7·44
8·00	1·17	4·98	124·5	3·76	14·90
10·6	2·12	5·23	130·9	3·96	7·60
11·6	2·22	5·25	131·2	3·98	7·78
9·35	1·29	5·02	125·5	3·77	14·80
19·70	2·72	5·34	133·5	4·06	—

tion-manholes without change of shape, and even bends forming chang[e] of direction in the flow could be designed so as to cause very little reductio[n] in the discharge.

Mr. Essex thought that engineers had been led astray by mathemati[-] cians who could have little knowledge and no practical experience on th[e]

Fig. 7.



subject; the problem of unaccelerated sewage-flow might be usefully examined on a small scale in the laboratory for pipes under pressure, and the National Physical Laboratory had rendered valuable service in enabling it to be shown that for pipes under pressure the Chezy coefficient C was a function of the Reynolds number, and, following the law of organic growth, might be written $C=A$ Log (Reynolds number). For gravitational flow in pipes not under pressure the correct solution would only be satisfactorily settled by rational argument amongst practical designers

after close and careful study of the abstract science of the subject. Consideration of the Chezy coefficient could never be abandoned, nor must the close relationship of that coefficient to the Reynolds number or to the Froude number be lost sight of. Mr. F. C. Scobey, on p. 18 of Bulletin No. 150 *, gave it as his considered opinion that "The variation of Kutter's n values for identical surfaces renders Kutter's formula unreliable when considered with a constant value of n"; what practical reliance could, therefore, be placed upon the formula used by the Author in the compilation of his Tables, when the inventor of the formula stated that it was based on Kutter's formula and gave a value of n between 0·012 and 0·013, while at the same time it could be seen that it gave a value of Chezy's C of $124\sqrt[6]{R}$? Mr. Essex had shown elsewhere ‡ how very unreliable Kutter's formula could be, and how limited was the application of any logarithmic formula of the type $h=\frac{fV^n}{R^y}$, especially where the sum of the indices $n+y$ was other than 3 and so could not comply with the law of similarity of flow laid down by Osborne Reynolds in 1883.

On p. 80 of Bulletin No. 150 *, Mr. F. C. Scobey had tabulated a list of the sum of those indices from different formulas dating from 1775 to 1927. The formula of Beigeleisen in 1914, where $n=1{\cdot}80$ and $y=1{\cdot}20$, and that of Williams and Hazen, where $n=1{\cdot}852$ and $y=1{\cdot}167$, most closely complied with the Chezy formula taking $C=A \log VR/\nu$. On p. 262 § the Author stated that "The accepted method of calculating the velocity and the discharge of any sewer or open channel, no matter what its form of cross-section may be, is by means of an expression of the following form:—$V=CM^xI^y$," and the formula he used was $h=0{\cdot}000065\,\frac{V^2}{R^{1{\cdot}333}}$ in which the sum of the indices was 3·333. That formula could not comply with the law of similarity of flow, and the Author's Tables would consequently prove to be misleading. In the following two pages he attempted to show in terms of radius what mathematicians had unquestionably led engineers erroneously to believe: namely, that area and discharge varied as the square of the diameter of the pipe, and that perimeter and hydraulic mean depth varied directly as the diameter. Although that might be sufficiently accurate to enable a tentative design of cross section to be obtained, some adjustment was needed. A little rational thought would explain that, for if the perimeter, when the pipe was half-full, were measured at the sides, the proportionate depth of flow in the centre would be bound to be something less than half the diameter, so that if the ratio P/R were to be retained at the usually accepted figure of 6·28 a

* "Flow of Water in Riveted Steel and Analogous Pipes." Washington, 1930.

‡ "Abstract Science and Academic Theory in Relation to Hydraulic Flow in Concrete Pipes and Culverts." Journal Inst. M. & Cy.E., vol. lviii (1931–32), p. 1863.

§ *Ibid.*

slightly larger area than half of the total area of the circular cross section would be needed. The commonly accepted theory that the same velocity existed in a pipe flowing half-full as in one flowing full appeared to be disapproved by the data published on p. 18 of Bulletin No. 854 of the U.S.A. Department of Agriculture *; practical engineers, however, realizing that Chezy's C was a function of VR/v knew that the discharge in cusecs was PVR, and that $Q/P=VR$ was a ratio which could be more accurately measured in the field than in the laboratory. Setting aside academic considerations, the following points were established:

(1) The discharge of the U-shaped section, calculated by any formula, exceeded that of the circular section by 12 per cent. when not quite full, and by 10 per cent. when full and under pressure.

(2) The U-shaped section never needed to become surcharged.

(3) The velocities set out in Table XVI showed a self-cleansing velocity of 1½ foot per second in the circular pipe for one-twelfth of the full capacity of 6 times the maximum dry-weather flow; at the latter discharge the circular sewer was full and under pressure, and anything above 6 times the maximum dry-weather flow was bound to find a storm relief on the town side of the sewage-disposal works. The Leatherhead culvert had been given a preference of 1½ inch in height over the 2-foot circular pipe, requiring only an addition of ½ cubic foot of concrete per yard run of culvert; the discharge had, however, been increased by a further 16 per cent., or by a total of 42 per cent. in excess of the maximum discharge of the 24-inch circular-section sewer.

(4) The question of future enlargement of a trunk sewer became less vital; an allowance being made for keeping all branch-connexions at only 1 foot above the top of the trunk sewer meant that by raising the slab only 12 inches at any future date the Leatherhead sewer could be increased in area to 5·55 square feet, the perimeter to 7·25 feet, and the hydraulic mean depth to 0·766 foot; the velocity would then be 3·55 feet per second, and the discharge would then be 19·70 cusecs, an increase of 70 per cent. for comparatively little extra expenditure and no trouble in dealing with the flow of sewage.

(5) The construction of U-shaped culverts permitted easy inspection during construction, and any defective workmanship became at once apparent.

(6) The initial saving in cost, compared with the circular section, would be at the least 10 per cent.

Mr. C. B. Lea, of Pretoria, thought that the chart reproduced as Fig. 8 (facing p. 418) might be found useful for determining the velocity in the

* D. L. Yarnell and S. M. Woodward, "The Flow of Water in Drain Tile." 1925.

sewer sections mentioned, when used in conjunction with the Tables at the end of the Paper. The chart was based on Manning's formula $v=\frac{1\cdot486}{n}R^{\frac{2}{3}}S^{\frac{1}{2}}$, which was the same as Santo Crimp's and Bruges's formula, when n had a value of 0·012. The chart was plotted on logarithmic paper and the lines running from the top left-hand corner to the bottom right-hand corner represented some value of nv. That product was divided by n by the lines running approximately at right angles, and hence the value of v could be determined. The ordinates represented the hydraulic mean depth in feet and the velocity in feet per second, both to the same scale. The abscissae represented the slope of the conduit. The following examples should make the use of the chart clear :—

Example 1. To determine the velocity in a conduit, the hydraulic mean depth (R) being 2·0, the slope being 0·01, and the coefficient of roughness (n) being 0·015.

First, the point of intersection of R=2·0 and S=0·01 should be found. From that point it was necessary to travel upwards parallel to the nv lines (shown dotted) until the line for n=0·015 was reached, and then to travel horizontally to the velocity scale, which gave the required velocity as 16 feet per second.

Example 2. To find the velocity in a sewer when R=1·0, S=1/1000, and n=0·012.

From the point of intersection of R=1·0 and S=0·001, it was necessary to travel upwards parallel to the nv lines. The left-hand margin was, however, met before the line for n=0·012, so that from the point of intersection with the margin (at 3·3 approximately) it was necessary to travel horizontally to the right-hand margin and then to continue upwards parallel to the nv lines until the required value of n was reached. In the example chosen the value of n was read on the broken lines in the right-hand bottom corner, and the velocity was about 3·8 feet per second.

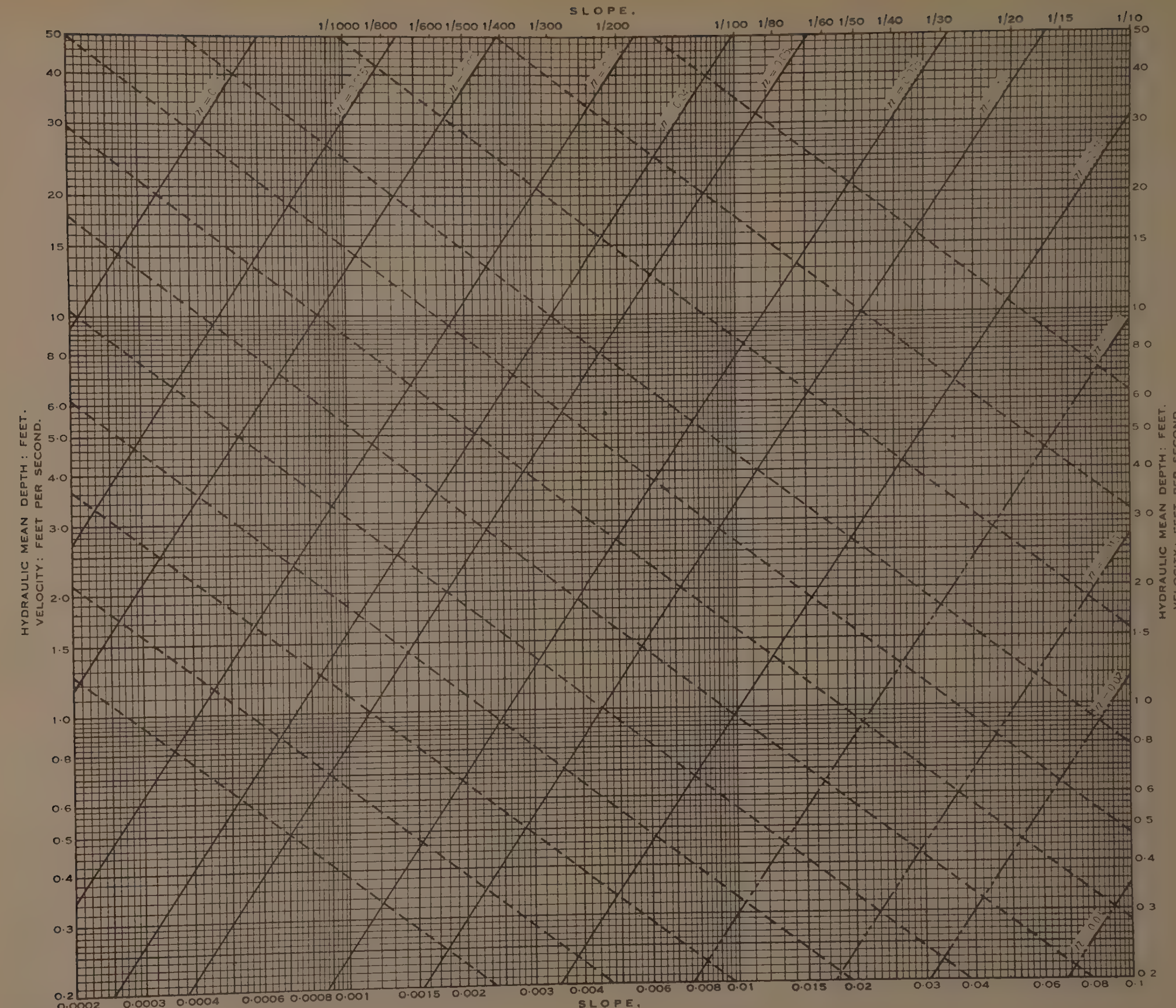
The chart had been originally prepared for designing storm-water channels, and was used in conjunction with a Table giving the values of A and R for various channels. In the case of an earth channel the maximum velocity was fixed, and knowing the discharge required, A was fixed. Then knowing the values of S and n the maximum value of R was found by working backwards on the chart. With A and R known it was then a simple matter to select a suitable channel. Similarly, from Table XVII (p. 418), the size of a pipe could be obtained for assumed conditions.[1]

[1] The values in Table XVII were obtained by slide-rule. A check of the figures by machine showed that, in general, the error was less than 1 per cent., but in a few cases it was nearly 2 per cent. The Table was intended for use with Fig. 8, and for that purpose the accuracy was ample.—C.B.L.

TABLE XVII.

Pipe-diameter: inches.	Depth of flow as a proportion of the pipe-diameter.									
	0·1	0·2	0·3	0·4	0·5	0·6	0·7	0·8	0·9	·
	Area: square feet.									
	$0·040d^2$	$0·112d^2$	$0·198d^2$	$0·293d^2$	$0·393d^2$	$0·492d^2$	$0·587d^2$	$0·673d^2$	$0·745d^2$	0·7
	R: foot									
	$0·063d$	$0·121d$	$0·171d$	$0·214d$	$0·250d$	$0·277d$	$0·296d$	$0·304d$	$0·297d$	0·2·
4	0·0044 0·0208	0·0122 0·0403	0·0216 0·0570	0·0319 0·0713	0·0428 0·0833	0·0545 0·0924	0·0639 0·0987	0·0733 0·1012	0·0812 0·0990	0·0 0·0
6	0·0100 0·0312	0·0280 0·0605	0·0495 0·0855	0·0732 0·1070	0·0982 0·1250	0·1230 0·1385	0·1466 0·1480	0·1682 0·1520	0·1863 0·1485	0·1 0·1
8	0·0178 0·0417	0·0498 0·0807	0·0882 0·1140	0·1304 0·1426	0·1749 0·1665	0·2190 0·1845	0·2610 0·1972	0·2990 0·2025	0·3320 0·1980	0·1 0·1
9	0·0225 0·0468	0·0630 0·0908	0·1114 0·1282	0·1650 0·1605	0·2210 0·1875	0·2770 0·2075	0·3300 0·2220	0·3790 0·2280	0·4190 0·2230	0·0 0·0
10	0·0278 0·0521	0·0777 0·1008	0·1375 0·1424	0·2035 0·1783	0·2725 0·2080	0·3410 0·2310	0·4070 0·2465	0·4670 0·2530	·05170 0·2473	0· 0·
12	0·040 0·063	0·112 0·121	0·198 0·171	0·293 0·214	0·393 0·250	0·492 0·277	0·587 0·296	0·673 0·304	0·745 0·297	0· 0·
15	0·063 0·078	0·175 0·151	0·310 0·214	0·458 0·267	0·615 0·313	0·770 0·346	0·918 0·370	1·051 0·380	1·164 0·372	1· 0·
18	0·090 0·094	0·252 0·182	0·446 0·256	0·659 0·321	0·885 0·375	1·105 0·416	1·320 0·443	1·515 0·456	1·675 0·446	1· 0·
21	0·123 0·109	0·343 0·212	0·607 0·299	0·899 0·375	1·203 0·438	1·507 0·485	1·800 0·518	2·060 0·532	2·282 0·520	2· 0·
24	0·160 0·125	0·448 0·242	0·792 0·342	1·170 0·428	1·570 0·500	1·970 0·554	2·350 0·592	2·693 0·608	2·980 0·594	3· 0·
27	0·202 0·141	0·567 0·272	1·000 0·384	1·480 0·482	1·990 0·565	2·490 0·623	2·970 0·666	3·410 0·684	3·770 0·669	3· 0·
30	0·25 0·156	0·70 0·303	1·24 0·428	1·83 0·535	2·46 0·625	3·07 0·693	3·67 0·740	4·21 0·760	4·66 0·743	4· 0·
36	0·36 0·188	1·00 0·363	1·78 0·513	2·64 0·642	3·54 0·750	4·43 0·832	5·28 0·888	6·05 0·912	6·70 0·892	7· 0·
42	0·49 0·219	1·37 0·423	2·43 0·598	3·59 0·749	4·82 0·875	6·03 0·970	7·20 1·036	8·25 1·063	9·14 1·039	9· 0·
48	0·64 0·250	1·79 0·484	3·17 0·684	4·69 0·856	6·29 1·000	7·87 1·107	9·40 1·184	10·77 1·217	11·92 1·188	12· 1·
54	0·81 0·281	2·27 0·545	4·02 0·770	5·93 0·964	7·95 1·125	9·95 1·246	11·89 1·332	13·63 1·368	15·09 1·336	15· 1·
60	1·00 0·313	2·80 0·605	4·95 0·855	7·33 1·070	9·83 1·250	12·30 1·384	14·68 1·480	16·82 1·520	18·63 1·485	19· 1·
66	1·21 0·344	3·39 0·666	5·99 0·941	8·87 1·178	11·89 1·375	14·88 1·523	17·75 1·628	20·35 1·672	22·55 1·634	23· 1·
72	1·44 0·375	4·03 0·727	7·13 1·025	10·55 1·284	14·15 1·500	17·72 1·661	21·13 1·776	24·22 1·824	26·82 1·782	28· 1·

FIG: 8.



UNIVERSAL FLOW-VELOCITY CHART.

The Author, in reply, observed that Mr. Essex referred to a U-shaped ewer constructed at Leatherhead by Mr. J. L. Davies in May, 1936, and hat he pointed out that the work, which had since been completed, showed nany advantages both during construction and in after use. The design s submitted by Mr. Davies was apparently regarded by the Ministry of Iealth as an entirely new departure in sewer-construction. Since that articular aspect of the case had been raised by Mr. Essex, the Author ubmitted the following statement to make the point entirely clear.

A scheme for the reconstruction and diversion of the Catherine Street nd Wellington Lane sewer, Sunderland, designed by the Author and ncluding 950 linear yards of from 36-inch by 36-inch to 48-inch by 48-inch J-shaped sewer, had been submitted to the Ministry of Health in February, 933, the Ministry signifying their approval in March of that year. The vork had been put out to tender in May 1933, and had been completed in une 1934, at a total cost, including subsidiary sewers, of approximately 27,000.

The U-shaped sewer constructed by Mr. Davies at Leatherhead was f form No. 1 (see p. 265 §), with tapering sides, measuring 25½ inches in eight by 21 inches in width. In the Author's opinion, although the Leatherhead sewer offered certain advantages with regard to self-cleansing elocities, the dimensions were too small to offer any economic advantages.

Reference to *Figs. 6* (p. 277 §) showing the comparative costs of circular nd U-shaped sewers, would, no doubt, make that quite clear.

Mr. Essex agreed that U-shaped sewers had very decided advantages ver other forms, and he stated that there appeared to be a great mis- onception with regard to those advantages. The Author, however, was naware of any general consensus of opinion with regard to U-shaped ewer design, and he hoped that the publication of his Paper would draw ttention to its undoubted advantages.

Mr. Essex suggested that the selection or assumption of a gradient in rder to determine the size of a sewer was a novel procedure, and whilst n certain instances the engineer might find his gradient already determined or him, the Author had very vivid recollections of laying down a series of rial gradients and of making comparative estimates from them, to deter- nine the most economical method of dealing with the particular problem n hand. Surely Mr. Essex did not suggest that in each and every instance sewer-gradient was so irrevocably determined that he could not exercise is skill in determining that combination of gradient and size which would esult in his sewer being constructed in the most economical manner?

Mr. Essex suggested that for a circular sewer a self-cleansing velocity f 1½ foot per second should be obtained at the proportionate depth of ·2, but the Author was unable to understand why that particular depth hould represent twelve times the minimum dry-weather flow when the

§ *Ibid.*

sewer was running to full capacity. The Author presumed that the se[illegible] cleansing velocity of 1½ foot per second at a proportionate depth of flo[illegible] of 0·2 applied to the average dry-weather flow for a sewer designed to car[illegible] twelve times the average dry-weather flow when full, as part of a foul-sew[illegible] system.

Using Mr. Essex's figures in Table XVI (pp. 412, 413) for a 24-inch cir[illegible] cular section, 0·4 foot of depth represented a proportionate depth of 0·[illegible] with a velocity of 1·53 foot per second, and a discharge of 0·684 cusec[illegible] which approximated to one-twelfth of the discharge when flowing full, th[illegible] was to say 7·74 cusecs.

It was further stated that if the self-cleansing velocity were to be 1½ fo[illegible] per second at a proportionate depth of flow of 0·20, then since $V/S^{\frac{1}{2}} = CR$[illegible] it was possible to determine the necessary diameter of sewer to fulfil tho[illegible] particular conditions.

It was quite true to say that to take a value of $C = 124R^{\frac{1}{6}}$ rath[illegible] than the value $C = 25 \log \frac{VR}{\nu}$ would result in a larger sewer-diameter, b[illegible] that showed the effect of employing different formulas and bore no relatio[illegible] ship to the form of cross-section.

With regard to the comment made on the statement relative to crow[illegible] gradients, the Author wished to point out that the crown of the sew[illegible] should properly represent the hydraulic gradient for which it was designe[illegible] whatever the form of cross-section might be, and for that reason he ha[illegible] used the crown as the basis for comparing costs.

Table XVI (pp. 412, 413) had been used to compare the relative di[illegible] charges of circular and U-shaped sewers. An examination of the Tabl[illegible] however, revealed that the calculated discharges for the 24-inch circul[illegible] sewer were for a gradient of 1 in 1,300, whilst those for the 21-inch U-shape[illegible] sewer were for a gradient of 1 in 1,090. In addition to that, the discharg[illegible] quoted by Mr. Essex in his remarks were for sewers of equal area. Th[illegible] Author, however, in making his comparison, had quite clearly indicate[illegible] that he compared sewers of equal diameter and equal gradients, where[illegible] Mr. Essex had compared sewers of equal areas, unequal diameters an[illegible] unequal gradients. It was quite evident, therefore, that any inferenc[illegible] drawn from Table XVI could not be used as a criticism of the figures give[illegible] by the Author in Tables I and III (pp. 280–281 §).

The Author had prepared Tables XVIII and XIX which showed th[illegible] properties of 24-inch diameter circular and U-shaped sewers at gradien[illegible] of 1 in 1,300, in accordance with the formula used by Mr. Essex to dete[illegible] mine the value of Chezy's C.

§ *Ibid.*

Table XVIII.—Circular Sewers.

Diameter: 24 inches. Slope: 1 in 1,300.
Area: 3·142 square feet. Hydraulic mean depth: 0·50 foot.
Velocity: 2·45 feet per second. Discharge: 7·70 cusecs.

Depth: feet.	Area: square feet.	Perimeter: feet.	Hydraulic mean depth: foot.	Velocity, V: feet per second.	Discharge: cusecs.	VR.	Log $\frac{VR}{v}$.	Chezy's C ($A=25$).
0·1	0·0580	0·898	0·064	0·617	0·0358	0·0395	3·506	87·7
0·2	0·1650	1·288	0·128	1·000	0·1640	0·1280	4·017	100·4
0·4	0·4480	1·853	0·242	1·530	0·6840	0·3700	4·484	112·0
0·5	0·6160	2·080	0·295	1·730	1·0600	0·5100	4·618	115·2
0·6	0·7760	2·281	0·340	1·920	1·4900	0·6550	4·726	118·0
0·8	1·1710	2·733	0·428	2·220	2·6000	0·9520	4·888	122·0
0·836	1·2390	2·810	0·441	2·260	2·8000	0·9970	4·909	122·9
1·0	1·5710	3·142	0·500	2·450	3·8600	1·2250	4·999	124·9
1·2	1·9710	3·550	0·555	2·610	5·1500	1·4500	5·071	126·7
1·4	2·3660	4·002	0·591	2·720	6·4400	1·6100	5·117	127·8
1·6	2·6940	4·430	0·608	2·770	7·4500	1·6850	5·134	128·2
1·8	2·9770	4·995	0·596	2·740	8·1500	1·6320	5·122	128·0
2·0	3·1420	6·283	0·500	2·450	7·7000	1·2200	4·999	124·9

Table XIX.—U-Shaped Sewers (Form No. 1).

Height: 24 inches. Breadth: 24 inches.
Slope: 1 in 1,300. Hydraulic mean depth: 0·500 foot.
Area: 3·571 square feet. Discharge: 8·784 cusecs.
Velocity: 2·46 feet per second.

Depth: feet.	Area: square feet.	Perimeter: feet.	Hydraulic mean depth: foot.	Velocity, V: feet per second.	Discharge: cusecs.	VR.	Log $\frac{VR}{v}$.	Chezy's C ($A=25$)
0·1	0·0580	0·898	0·064	0·617	0·0358	0·0395	3·506	87·7
0·2	0·1650	1·288	0·128	1·000	0·1640	0·1280	4·017	100·4
0·4	0·4480	1·853	0·242	1·530	0·6840	0·3700	4·484	112·0
0·5	0·6160	2·080	0·295	1·730	1·0600	0·5100	4·618	115·2
0·6	0·7760	2·281	0·340	1·920	1·4900	0·6550	4·726	118·0
0·8	1·1710	2·733	0·428	2·220	2·6000	0·9520	4·888	122·0
1·0	1·5710	3·142	0·500	2·460	3·8640	1·2300	5·000	125·0
1·2	1·9710	3·542	0·556	2·617	5·1580	1·4500	5·070	126·5
1·4	2·3710	3·942	0·601	2·754	6·5290	1·6500	5·130	128·2
1·6	2·7710	4·342	0·638	2·821	7·8200	1·8000	5·170	129·2
1·8	3·1710	4·742	0·669	2·953	9·3700	1·9700	5·200	130·0
*2·0	3·5710	7·142	0·500	2·460	8·7840	1·2300	5·000	125·0
2·0	3·5710	5·142	0·695	3·020	10·7800	2·1000	5·240	130·8

* Closed top.

The following comparison was then possible :

Mr. Essex.	Author.
Atmospheric discharge : 24-inch circular section.* Fall 1 in 1,300. Discharge 8·15 cusecs. 23-inch by 21-inch U-shaped section.* Fall 1 in 1,090. Discharge 10·00 cusecs. Excess capacity of U-shaped section, 22·60 per cent. Mr. Essex quotes this as 12·25 per cent.	24-inch circular section. Fall 1 in 1,300. Discharge 8·15 cusecs. 24-inch by 24-inch U-shaped section (form No. 1). Fall 1 in 1,300. Discharge 10·78 cusecs. Excess capacity of U-shaped section 32·2 per cent. Author's Tables I and III give 32·70 per cent. on Santo-Crimp formula.
Drowned discharge : 24-inch circular section.* Fall 1 in 1,300. Discharge 7·74 cusecs. 23-inch by 21-inch U-shaped section.* Fall 1 in 1,090. Discharge 8·00 cusecs. Excess capacity of U-shaped section, 3·40 per cent. Mr. Essex quotes this as 10·30 per cent.	24-inch circular section. Fall 1 in 1,300. Discharge 7·70 cusecs. 24-inch by 24-inch U-shaped section (form No. 1). Fall 1 in 1,300. Discharge 8·78 cusecs. Excess capacity of U-shaped section 14·0 per cent. Author quotes 13·8[illegible] per cent. on Santo-Crimp formula.

* Sections of equal area.

The comparisons quoted above clearly indicated that the Author's Tables, based on the Santo-Crimp formula, gave an accurate indication of the comparative values of the sections considered. In addition to that, it was clear that the application of Mr. Essex's method of calculation gave results which agreed to within 0·50 per cent. of the Author's Tables.

The adoption of a U-shaped section undoubtedly resulted in advantages with regard to self-cleansing velocities and permissible overloads before surcharging took place, together with uninterrupted passage of flows through manholes, with consequent improved efficiency of the sewer as a whole, and the Author was pleased to note that Mr. Essex appreciated those points.

Mr. Essex had discussed at some considerable length the relative merits and demerits of the formulas used in calculating discharges, both in pipes and in open channels, and whilst it was quite true to say that there were considerable discrepancies to be found between discharges calculated by different formulas, it was equally true that the selection of a formula was surely a matter for individual choice and inclination.

Mr. Essex proceeded to infer that the Author's choice of the Santo-Crimp formula was an unhappy one, and he then proceeded to quote Reynold's law of resistance, which he used to calculate the value of Chezy's

, and he then determined velocities and discharges by means of the old hezy formula.

The formula used by Mr. Essex for design in sewers was apparently $= 25 \log \frac{VR}{\nu} . R^{\frac{1}{2}}S^{\frac{1}{2}}$, where V denoted the average velocity, R the hydraulic ean depth, S the inclination or slope, and ν the coefficient of kinematic iscosity. Alternatively, the Santo-Crimp formula could be re-stated as $= 124R^{\frac{1}{6}}.R^{\frac{1}{2}}S^{\frac{1}{2}}$.

Upon the variation of those two formulas Mr. Essex sought to prove hat the Tables prepared by the Author were misleading, and to do so he hade reference to Papers written by himself and by Mr. F. C. Scobey. t appeared to the Author that Mr. Essex had confused the relative merits f forms of cross-section with the relative merits of different formulas. The riticism put forward by Mr. Essex had no foundation in fact, and the uthor wished to express the opinion that the application of any particular ormula affected only the actual size of sewer selected, and did not in any ay affect relative merits of one form of section with regard to another. he Author wished to refer Mr. Essex to the following statement which ppeared on p. 267 §: "It must be noted, however, that the powers to hich the hydraulic means depths and the diameters are raised do affect hese comparisons. The actual effect on the relative sizes of each particular ection is, however, a small one, and since the Santo Crimp and Bruges ormula is the one most generally used when considering sewer-sections, t is proposed to make the comparisons on that basis."

In summing up, Mr. Essex had enumerated certain points with regard o the Paper, and for the sake of clarity, the Author appended his remarks n the same order :—

(1) The discharge of the U-shaped section when flowing full exceeded that of a circular section of equal diameter by 13·8 per cent., the maximum discharge of a U-shaped sewer being 32·70 per cent. in excess of the maximum discharge of a circular sewer of equal diameter.

(2) It was not true to say that a U-shaped section never needed to become surcharged; indeed, unfortunately, by the very nature of things, there was always the possibility of any sewer becoming surcharged if the outlet were drowned. It was true to say, however, that the U-shaped section allowed a larger factor of safety in that regard than either a circular or an egg-shaped section.

(3) Mr. Essex again emphasized the advantages to be obtained from a U-shaped section with regard to dry-weather flows. His point with regard to the increase in capacity by the increase in height of the sewer was clearly indicated in *Fig. 1* (p. 268 §).

§ *Ibid.*

(4) The advantages to be obtained by increasing the height of the U-shaped section were very evident, and the Author was pleased to say that a scheme for the construction of Cut-throat Valley sewer, in Sunderland, with allowance for future enlargement, had been submitted to, and had been approved by, the Ministry of Health in 1936. The preparation of this particular scheme had involved the consideration of an initial drainage area of 619 acres, and a future drainage area of 1,18 acres, the selection of the two hydraulic gradients used involving very careful consideration.

(5) The Author found it difficult to believe that the construction of U-shaped sewers permitted easier inspection, unless they were of unusually small dimensions.

(6) The percentage savings in capital costs were shown in *Fig.* (p. 277 §), and were set out on p. 278 §.

Mr. C. B. Lea showed in Fig. 8 (facing p. 418) a chart based on an ingenious logarithmic plotting of the Manning formula. The method of plotting was particularly apt, as the values of Kutter's n were clearly indicated, thus enabling the chart to be used for almost any type of channel. The properties of particular sections, as shown in Table XVII (p. 418) could be prepared from the Author's Tables, and used in conjunction with Mr. Lea's diagram.

For some years past the Author had used a chart based on a logarithmic plotting of the Santo Crimp formula for sewer design. The Author's chart enabled the diameter and velocities of sewers to be read off when the values of the slope and capacity were known.

The Author wished to acknowledge the kindly advice and encouragement given to him by Mr. J. E. Lewis, Assoc. M. Inst. C.E., the Borough Engineer of Sunderland, with regard to the preparation of his Paper.

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§ *Ibid.*

Paper No. 5082.

"The Flow of Water in Short Channels." †

By Charles Frederick Jason Lisle, B.Sc., Assoc. M. Inst. C.E.

Correspondence.

Mr. E. H. Essex observed that he had already shown * that the use of ι formula of the type $N=f\frac{V^x}{R^y}$ could not comply with dimensional simili-ude unless the sum of the values x and y was 3. It could be shown that he Author's curve for type III in *Fig. 2* (p 308 §) for the critical depth vas altogether inaccurate, and, in fact, it was bound to be so, if only on ιccount of its being based upon the logarithmic formula of Mr. A. A. Barnes, ıamely, $V=92{\cdot}1R^{0{\cdot}602}S^{0{\cdot}466}$, or $S=0{\cdot}000063\frac{V^{2{\cdot}15}}{R^{1{\cdot}29}}$, the sum of the indices : and y of which was 3·44. Mr. Essex did not like any of the Barnes ormulas because it was tedious work comparing them with the value of C in the Chezy formula. The best logarithmic formula to use for earth channels, if such a formula had to be adopted, was that of Professor Γ. Claxton Fidler, namely $N=A\frac{V^{2{\cdot}1}}{R^{1{\cdot}5}}$, where V varied as $R^{0{\cdot}60}$. Mr. Essex ıad used it with slide-rule calculations to compile Table XIII (p. 426) for comparison with Table I on p. 293 §. Table I showed what would happen n a channel of type I (Appendix I, *Fig. 2* (p. 308 §)) having a fall of 1 in 1,000 with a discharge of 900 cusecs—that was to say, a channel not in regime. Table XIII, however, showed that the critical depth of the same channel was 7·5 feet for a discharge of 900 cusecs if laid to a fall of 1 in 1,000, and also showed what would be the critical velocities and discharges at various depths for the same fall.

Table I showed that the channel required a velocity of 10·6 feet per second at a depth of 4·9 feet—quite an inadmissible velocity in an earth channel of the type under discussion.

On p. 307 § the Author expressed the hope that engineers who had available the results of measurements of non-uniform flows on full-size works would make such measurements known in the Correspondence. Mr. Essex was not clear as to what advantage was to be gained by the calculation of non-uniform flows in channels, but if agreement were reached upon the correct method of designing channels for uniform flow,

† Journal Inst. C.E., vol. 7 (1937–38), p. 287 (December 1937).

* *Correspondence on Paper on* "The Effect of the Form of Cross-section on the Capacity and Cost of Trunk Sewers," by Mr. T. Donkin; p. 410, *ante*.

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

TABLE XIII.

Fall of Channel 1 in 1,000; Chezy $C = 13{\cdot}7 \log VR/\nu$; Froude number $= 0{\cdot}433 \log VR/\nu$.

Depth of flow: feet.	Area: square feet.	Perimeter, P: feet.	Hydraulic mean depth, R: feet.	$R^{\cdot 060}$	V: feet per second.	Q: cusecs.	VR.	$\log VR/\nu$	C.	$V/\sqrt{R}$.	From Barnes's formula.	
											C.	Gradient required: C^2R/V^2.
7·5	159·5	37·1	4·30	2·40	5·65	900	24·3	6·29	86·2	2·72	78·0	825
7·0	143·8	35·3	4·07	2·32	5·46	785	22·3	6·26	85·8	2·71	77·0	810
6·5	128·5	33·5	3·84	2·24	5·28	680	20·3	6·21	85·2	2·69	76·0	800
6·0	114·0	31·7	3·60	2·16	5·09	580	18·3	6·17	84·6	2·67	75·5	800
5·8	108·5	31·0	3·51	2·12	5·00	542	17·5	6·15	84·3	2·66	75·0	800
5·6	103·0	30·2	3·41	2·09	4·92	507	16·8	6·14	84·1	2·65	74·5	790
5·4	97·8	29·5	3·32	2·06	4·85	474	16·1	6·12	83·8	2·65	74·0	780
5·2	92·6	28·8	3·22	2·02	4·76	441	15·3	6·09	83·6	2·64	73·5	775
4·9	85·0	27·7	3·07	1·96	4·61	392	14·2	6·06	83·2	2·63	73·0	770

the measurement of friction-loss for excess flows in a channel was bound to follow on the same lines; in that respect he thought that Mr. Gerald Lacey's Paper * should be studied; the only drawback to that Paper was that the calculations were based on Manning's logarithmic formula, modified by Mr. Lacey from $C=\frac{1{\cdot}486}{N}\sqrt[6]{R}$ to $C=\frac{1{\cdot}3458}{N_a}\sqrt[4]{R}$ when VR was unity. That modification complicated the consideration of that Paper, but if Mr. Lacey's Table II on p. 430 of his Paper * were further modified and extended to comply with the simple Chezy formulas as shown in Table XIV opposite, Mr. Lisle would be able to make a comparison therewith to enable him to derive a basis for the correct measurement of frictional loss.

Table XIV gave concisely and fairly a summary of Mr. Lacey's views on friction-loss, and of his general views of the rules governing the hydraulic flow in streams carrying silt; as such the figures might supply the answer to Mr. Lisle's query.

Mr. W. H. R. Nimmo, of Brisbane, observed that the Author expressed the hope that engineers would make known any measurements of non-uniform flows in full-size works. That the behaviour of large quantities of water under various conditions of non-uniform flow could be predicted with reasonable accuracy by calculation had been confirmed by experiments made in 1923 by Mr. Julian Hinds †, and by those carried out by Mr. Nimmo in 1922–23 ‡. The correctness of the theoretical results obtained had been supported by numerous practical applications made during recent years.

* "Uniform Flow in Alluvial Rivers and Canals." Minutes of Proceedings Inst. C.E., vol. 237 (1933–34, Part 1), p. 421.

† "Side Channel Spillways: Hydraulic Theory, Economic Factors, and Experimental Determination of Losses." Trans. Am. Soc. C.E., vol. 89 (1926), p. 881.

‡ "Side Spillways for Regulating Diversion Canals." *Ibid.*, vol. 92 (1928), p. 1561

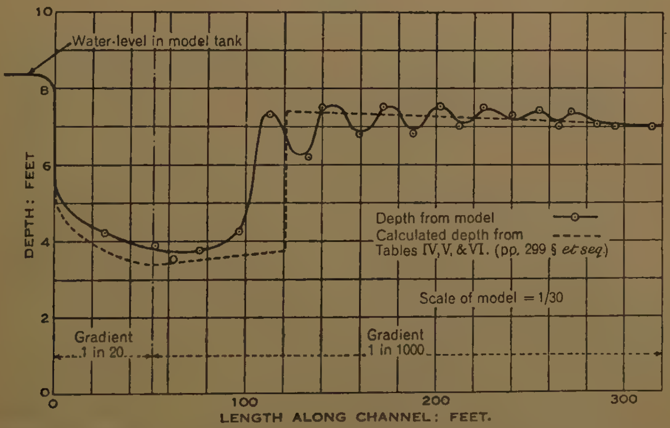
TABLE XIV

NOTE.—Log $VR/\nu = 5$, at $VR = 1$ for water at a temperature of 70° F.

N_a (Lacey).	N (Manning).	Chezy value at $VR=1$: $C = A \log \frac{VR}{\nu}$.	$A = C/\log \frac{VR}{\nu}$.	Froude No. $V/\sqrt{R} = C\sqrt{S}$ / Silt-factor c'. / $E_x \log VR/v$.	Efficiency factor E for $V/\sqrt{R}$.	$(E/A)^2 = S$.	$1/\sqrt{S} = A/E$.	Lacey's silt-factor: $0.75\,(c')^2$.	Diameter of silt-grains: inches.
0·0150	0·0165	90	18	0·512	0·1024	0·0000323	176	0·200	—
0·0175	0·0193	77	15·4	0·697	0·1394	0·000082	110	0·365	0·0028
0·0200	0·0221	67	13·8	0·910	0·1820	0·000174	76	0·622	0·0055
0·0225	0·0248	60	12·0	1·151	0·2302	0·000369	52	1·000	0·016
0·0250	0·0276	54	10·8	1·422	0·2840	0·000690	38	1·530	0·040
0·0275	0·0304	49	9·8	1·720	0·3440	0·001230	28·5	2·240	0·080
0·0300	0·0330	45	9·0	2·047	0·4094	0·002070	22	3·160	0·155
0·0400	0·0442	33·5	6·7	3·550	0·7100	0·0112	9·45	9·500	1·55
0·0435	0·0480	31	6·2	4·280	0·8560	0·0191	7·25	13·80	3·00

Dr. Alexander Thom thought that it might be of interest to record an experiment, made in the James Watt Engineering Laboratories in

Fig. 11.



Glasgow, to verify the method used in the Paper to calculate the position of the hydraulic "jump." A model of the channel discussed in Example III (pp. 298 § *et seq.*) was made to a scale of one-thirtieth full size. The flume used was therefore 4 inches wide, and had a slope of 0·05 for the first 21 inches and a slope of 0·001 thereafter. The discharge of the channel being 700 cusecs, that of the model should be $700 \div 30^{\frac{5}{2}}$,

§ *Ibid.*

namely 0·142 cusec. The level in the tank was accordingly adjusted until that discharge was obtained. It was found necessary to round the entrance slightly to reduce the diagonal standing waves on the shooting flow in the first section of the flume. The depths were then measured at a number of points. They were shown in *Fig. 11* reduced to full scale, along with the calculated depths as in *Fig. 7* (p. 300 §). It would be seen that in the model flume the main standing wave was followed by a series of smaller waves, but that otherwise the agreement was as good as could be expected considering the somewhat rough agreement between the experimental conditions and those assumed in the calculations. The flume used was of planed wood painted with a bitumastic paint, and it had not necessarily the same coefficient of friction as that assumed by the Author.

The Author, in reply, stated that, with regard to the term "the critical depth," it appeared to him that Mr. Essex had confused it with "the critical velocity," which latter term was used to denote that velocity at which a flow ceased to be a streamline flow, or sometimes, in cases of river-flow, that velocity at which sediment commenced to move on the river-bed. By studying the definition of the critical flow given on p. 289 § it would be seen clearly that no friction-formula had been used in compiling the Tables from which the curves in *Fig. 2* (p. 308 §) were plotted.

The subject of the choice of a friction-formula was always a controversial one, and should be left with the engineer who had a knowledge of the particular features of the channel in question. The Author did not agree that a formula should be condemned for the sole reason that it was tedious work comparing it with the value of C in the Chezy formula. He had found, however, in the calculations for non-uniform flow—which were nearly all of short length—that the type of friction-formula used made a very small difference to the final calculated depths.

Mr. Essex cast some doubt on the advantage to be gained by the calculation of non-uniform flows. Such advantage was clearly to assist the designer to combine economy with safety, and to analyse existing works with a view to what would be the results in the event of a flood in excess of any experienced before. Such calculations would also save much time and expense in the building of scale models to ascertain the effects of peculiarities of a design. That was borne out by the results of the experiment on a model recorded by Dr. Thom, in so much as the recorded results were in general agreement with the calculated ones. The coefficient of friction of the model was evidently considerably less than that assumed in the calculations, and that would have the effect of throwing the hydraulic jump slightly upstream as, in fact, was the case. The appearance of the subsidiary waves following the main standing wave emphasized the advantage of following up calculations by small-scale models whenever possible.

§ *Ibid.*

CORRESPONDENCE
ON PAPERS PUBLISHED IN
JANUARY 1938 JOURNAL.

Paper No. 5150.

" The Reconstruction of Chelsea Bridge." †

By Ernest James Buckton, B.Sc. (Eng.), and
Harry John Fereday, MM. Inst. C.E.

Correspondence.

Mr. S. R. Banks, of Montreal, entered the controversial field of æsthetics with some diffidence, realizing that the design of the new Chelsea bridge had been the subject of much study on the part of eminent engineers and architects before it had been finally evolved in its present form. He considered that the somewhat severe aspect of the bridge was a fine expression of the modern trend towards purely functional design. He had, incidentally, compared photographs of the Chelsea bridge and the similarly self-anchored Cologne-Mülheim bridge, and he had been impressed by the much more graceful outline of the first-mentioned structure due to the loading of the cables in the shore-spans. There were one or two details of the general appearance of the bridge, however, on which he would like to comment.

He had never been able to reconcile himself to the appearance of the hinge at the bottom of a rocker-tower. He felt that the " knife-edge " suggested by that articulation was not in keeping with that conception of stability which was properly associated with the main supporting members of a suspension-bridge. The appearance of the hinge evoked the idea of motion, whereas that of the heavy member supported thereon gave the impression of repose. Undue emphasis thus came to be placed on what was, after all, only a secondary function of the post when compared with its primary duty as a column. It might be argued that a deliberate masking of that pin-joint would be unethical: on the other hand, however, he thought that a similar argument could be applied to the case of the stiffening-girder hinges, where no attempt at the exhibition of a similar function had been thought necessary, and to the relegation of the sway-bracing of the towers to a location below the bridge-floor where its presence was not apparent to the eye.

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† Journal Inst. C.E., vol. 7 (1937–38), p. 383 (January 1938).

Another debatable point, in his opinion, arose in connexion with the saddles at the tops of the posts. At those places, owing to the highly efficient use of steel in almost pure tension in the cables, and to the much lower compressive stresses necessarily obtaining in the main posts, there was a great disparity between the outside dimensions of those two members, although they were both carrying loads of the same order. In the present instance, he thought that the rather crude appearance of the exposed saddle-casting and its cover did not conform in a very satisfactory manner with the unusually clean lines of the post and the inherent grace of the cable-curve. While he agreed with the general rule that the employment of anything in the nature of applied ornamentation was to be deprecated, he felt, nevertheless, that some carefully-chosen architectural feature could be developed which would enable those two very different types of structural member to be brought together more harmoniously at their conjunction.

In reference to the construction of the suspension-cables, he was particularly interested in the choice of locked-coil strands in preference to strands made from the more conventional round wires, and in the reasons given for that choice. In departing from the simplicity and perfection of the circular section of material, there was entailed some sacrifice of tensile and bending strength as well as of the general robustness of the wires, and this loss had to be compensated for by the use of a larger area of steel in the cables. The strand, too, he noted from the computation in Table I (p. 394 §), had an expected efficiency (that was to say, a ratio of ultimate strength of strand to aggregate ultimate strength of component wires) of only 90 per cent. He imagined also that more than ordinary care had had to be exercised in the manufacture (which was a highly-specialized process), socketing, pre-stressing, and erection of the strands. He would be interested to know if such had actually been the case, and whether, for instance, the diameter of the strand-reels had had to be larger than it would have been in the case of ordinary strands.

With regard to the very important matter of prevention of corrosion, he felt that the additional protection afforded by the filling of the voids and interstices of the cables with a bituminous compound was to some extent offset by the considerable weakening of the galvanizing which took place during the extra "drawing" that was necessary to bring the wire-gauges within the close tolerance required for the accurate closure of the locked-coil strand. Would the ultimate durability of that bituminous covering be superior to that of heavy galvanizing? In connexion with the amount of corrosion to be anticipated in a bridge-cable, he thought it would be of interest to quote from a recommendation (advocating the use of wire-cables in preference to eye-bar cables) made by the Board of

§ Page number so marked refer to the Paper. (Journal Inst. C.E., vol. 7 (1937–38), p. 383 (January 1938)).—SEC. INST. C.E.

Engineers responsible for the design of the Delaware River bridge, U.S.A.†:

"The wire in all instances, where the maintenance was efficient, shows no deterioration after years of service. When the Niagara Suspension Bridge * was taken down, because of increase in loads from heavier rolling-stock, the wires, after about fifty years of service, were found to be bright and when cut apart tended to return to the original coil. They were not galvanized.

"In the existing Williamsburg Bridge * in New York the wire is found to be in very good condition generally through the entire thickness of the $18\frac{5}{8}$-inch cable, although the wire was not galvanized.

"The wires of the Brooklyn * and Manhattan * Bridges were galvanized and on recent examination were found in excellent condition. The Brooklyn Bridge cables have been in service for at least forty-four years."

The Authors had referred to the maintenance of a pre-determined overall diameter for the cable so that it would fit the tower-tops and other cable-castings. In similar work with which he had been associated in Canada, the procedure had been to allow the rope-maker a somewhat greater tolerance in strand-diameter, and to base the design of the effected members on a nominal size of strand and cable. Later, after the actual manufactured strand-diameter had been determined from a large number of measurements, the steelwork-contractor would be provided with the final inside dimensions of the saddles and clips, and could then proceed with their manufacture.

Owing to the comparatively short length of the cable in the case of the Chelsea bridge, it had been possible to carry out the operations of pre-stressing and measurement of the strands in an enclosed building, and under what seemed to have been ideal circumstances. In view of that, he regretted that more detailed information regarding the results of those processes had not been given in the Paper. Perhaps the Authors would be able to give some figures relating to the average modulus of elasticity of the strand before and after pre-stressing, the amount of permanent lengthening that had occurred during that operation, and the degree of accuracy that had been obtained in the measurement of length. He would also be very interested to know if the phenomenon of strand shortening due to reeling-up subsequently to measuring had occurred, and, if so, to what extent.

Referring to the strand-sockets, he considered that their design, with an adjustable bearing-sleeve, was both neat and efficient. The provision

† The Delaware River Bridge Joint Commission: Final Report of the Board of Engineers, 1927.

* The bridges mentioned in the above statement were erected in the years 1854, 1903, 1883 and 1909 respectively.

of a spherical bearing-surface was a feature which might with advantage be reproduced frequently in the anchorages of suspension-bridges, in order to obviate not only the effects of deliberate variations from normal alignment (as in the present case), but also those due to incidental small errors in the fabrication or assembly of the various parts of the anchorage system.

A point which seemed to him to be worthy of comment was the divergence between British and North American practice in regard to the socketing of wire ropes. That was exemplified in the use of a white-metal alloy in the former case, and of commercially-pure zinc in the latter case. Another difference was that, in British practice, some of the broomed wires were turned back upon themselves, whilst in Canada and the United States it was customary in bridge-work to leave all the wires straight (except for a slight outward bend in the case of the outer wires, in order to ensure that there would be room for the zinc to run between them and the inside face of the socket). It was also usual in those two countries to provide some device to prevent any accidental movement of the soft metal cone inside the socket during the handling of the rope or strand; and in many cases the inside of the socket was tinned previously to pouring. Again, it was quite a common procedure to clean the broomed ends of the wires with either an acid or a caustic solution, whilst the use of a flux such as resin was not general. In view of those differences, there seemed to be considerable scope for research in order to establish the relative merits of various methods of attachment of sockets.

He observed that hanger-rods had been used in preference to wire-rope hangers, and he presumed that the prevention of corrosion had been a major factor in arriving at the decision. He was curious to know the reason for the provision of adjustment in those hangers, since, with the accurate means of measurement available nowadays, equal or greater precision could in all probability be obtained by the omission of such adjustment. Further, he thought that the rods would have had a neater appearance in the absence of the turnbuckles. The employment of a wedge-piece inside the hanger-clip was an extremely ingenious method of preventing the clip from slipping under load over the smooth surface of the cable, and the separate gusset-plate of varying shape was also a valuable feature of that important detail.

He had confined his observations to the steelwork of the bridge, but he would like, in conclusion, to comment upon the remarkably clean and satisfying lines of the piers and the abutments, and also upon the valuable experimental data that had been collected with regard to the behaviour under loads of the London clay upon which the piers were founded.

Mr. G. A. Maunsell observed that the decision to make use of locked-coil wire ropes for the main cables of the bridge was interesting, and the reasons given by the Authors for the employing of those particular ropes had no doubt been very carefully considered before the decision was

ıade. The locked-coil wire rope might be said to represent the highest evelopment of the rope-maker's art. Difficulties had been experienced t one time on account of a tendency of the outer ring or sheath of locked egments to slip longitudinally relatively to the inner rings or core of the ope, but manufacture had of recent years been brought to such a pitch f perfection that that difficulty had been overcome, and there were in Great Britain makers who were able to turn out a really reliable rope of hat kind. It had mainly been evolved as a winding rope for mining vork, or as a running rope for aerial tramways. The specially-shaped vires of the outer and middle rings fitted together much more compactly han round wires. The fill-factor in locked-coil ropes amounted to about 0 per cent., as compared with a fill-factor of 75 per cent. in a spirally-laid trand of round wires, and the high fill-factor enabled a rope of given liameter to contain a maximum of metal, and therefore of strength. Where the size of winding drums was of importance, as in deep mine-shafts, the more compact size of the locked-coil rope was an advantage. Another advantage which it possessed for both winding and aerial-ropeway purposes was the fact that if one of the outer wires broke, it would be ocked by its neighbours and could not unwind. Two or three neighbouring wires had to break close together before unwinding could occur. An advantage which that wire possessed for ropeway use was the smooth circular outer periphery forming the best track for the loaded trolley-wheel.

Generally speaking, the locked-coil rope was admirable for use in situations where it was subjected to a heavy load, to surface-wear, to high-speed movement, and to considerable flexure. It was questionable, however, whether that kind of rope offered any advantages for suspension-bridge cables, because in the latter situation there was practically no flexure, no movement, and no wear on the rope. It became, therefore, a very moot point whether the extra cost of that type of rope was worth while. In a bridge such as the one under consideration there was a surprisingly small tonnage of steel in the wire-rope cables, however, and the extra cost of the rope might therefore not have had to be considered so carefully as in a bridge of larger span.

It would be informative if the Authors would say how many tons of structural steel had been used in the entire bridge, and also how many tons of wire-rope cable. According to a rough calculation there would appear to have been about 230 tons of wire rope in the main cables, and on the assumption that £20 per ton extra were paid for the luxury of locked-coil ropes, the total extra expenditure on that account would only have been of the order of £5,000, which was small compared to the whole cost of the work. Perhaps the Authors would give the actual figures.

The pre-stressing of ropes added from £10 to £15 per ton to the cost of the ropes. "Pre-stressing" was a modern word, but it meant little more than taking some of the initial stretch out of the rope before erection.

Rope manufactured in a good modern factory varied very little, if at a in the matter of the amount of initial stretch, and one sample of rop would be found to stretch almost exactly the same amount as anoth sample of the same brand. There might, therefore, be nothing very muc to be gained by pre-stressing from the point of view of scientific exact tude; in some types of construction, however, and perhaps the preser case was one, the elimination of stretch beforehand made the work erection rather more straightforward and simple. It was a practice whic had to be considered on its merits in any individual case.

It was rather curious to find wedges being inserted underneath four the thirty-seven ropes in the bundle with the intention of forming a enlargement of the cable to prevent slip of the cast-steel suspender-clamp because the insertion of those wedges was bound to have made it extremel difficult as a practical measure to ensure that just that amount of extr length was provided in just the right position in the ropes affected t enable them to take their part in the bundle without being either slack or tighter than their neighbours; in fact, he remembered noticing, whe he had seen the bridge under construction, at least one place where th ropes which passed outside the wedges appeared to be quite slack or loos as compared with the other ropes. Would the question of the use c those wedges be reconsidered in a future design?

Dr. H. J. Nichols, of Bombay, had been greatly interested in th economic features of the design, but he had had difficulty in extractin from the Paper information which enabled the main characteristics c weights, stresses, deflexions, etc., to be calculated for comparativ purposes.

The first point which impressed him was the comparatively hig bending stresses in the stiffening girder with the centre and side span fully loaded. It appeared, in fact, from the Paper that under thos conditions about 10 per cent. of the total loading was carried by th stiffening girder acting as a simple beam 340 feet long, and 90 per cent by the cable; the stiffening girder at the centre was accordingly sub jected to a compressive stress of about −6·4 tons per square inch, plu bending stresses of −4·2 tons per square inch in the top flange and o +5·8 tons per square inch in the bottom flange. What did the Author consider to be the permissible working stress for members such as thos subjected to combined compression and bending? A member of thos proportions ($\frac{l}{r}$=about 80) under longitudinal compressive stress alon would be permitted by the secant formula to carry −7·3 tons per squar inch for high-tensile steel, and 80 per cent. of that figure for mild steel.

It would appear that the necessity for proportioning the stiffenin girder to act as a strut (the depth/span ratio appeared to be 1/40, as com- pared with 1/82 for the Island of Orleans bridge) had resulted in a girde which was deeper than was really convenient for the cable-sag of 1/8·8, and

ossibly required that the working stress in the cables, which from p. 392 § as only about 12·9 tons per square inch, should be kept low so as to void excessive deflexions and still higher bending stresses in the stiffening rders. The deflexions at the centre under design live loads appeared range between +11 inches and $-8\frac{1}{2}$ inches, and at the quarter-points r the centre span only (partially loaded), between $+7\frac{1}{2}$ inches and $-\frac{1}{4}$ inch. It appeared that the ratio of maximum live load to dead load as about 0·6, which seemed rather low for that span.

The ratio of effective area to overall cross-sectional dimensions in the opes appeared to be about 0·92, and the net aggregate area of the cables as about 100 square inches. Could the Authors give the stress–strain elationship of the ropes after stretching? He avoided the term "pre-tressing" because he thought it did not describe very accurately the rocess of stretching a wire rope, and it further led to confusion with a eaning more usually attached to the word.

Could the Authors say, in assessing the stiffness of the stiffening irders, what allowance had been made for the numerous structural details uch as covers, splice-plates, stiffeners, etc., which were worked into those irders? Could not some advantage have been obtained by lowering the osition of the pins in the stiffening girders at the ends of the centre span, o as to equalize more closely the maximum bending loads in the top and ottom flanges of the girders, and also to avoid the necessity for adding teel to the top flanges? The neutral axis of the girder, as far as could e estimated from Figs. 12, Plate 2 (facing p. 446 §), was about 10 inches bove the centre-line, and that distance appeared to fit the stress-lines in *Fig. 11* (p. 398 §). The deck-plating appeared to be about 14 inches elow the neutral axis.

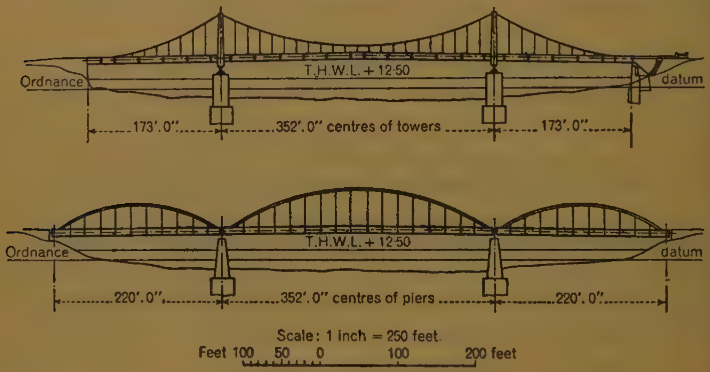
On p. 386 § the Authors mentioned that a reduction of impact had een permitted to make allowance for the maximum probable loading on ortions of the structure with more than one line of traffic. In calculating he maximum bending moments in the stiffening girders it was usual to assume that the prescribed intensities of loading covered certain portions of the span while the rest of the span remained entirely unloaded. That was a condition which was most unlikely to occur in practice, particularly in the case of a six-lane bridge. Would not a relaxation in that direction be justified no less than a reduction of impact in the manner mentioned?

The low working stresses in the cables suggested that little extra metal at a lower unit price would have been required had the cables been inverted and used in compression to form three stiffened arch spans, as indicated in *Figs. 29* (p. 436). The rise of the centre-span arch would be 25 per cent. greater than the sag of the cable. With the number of hangers shown the buckling length of the arch ribs would have been reduced to about one-seventeenth that of the original stiffening girders, which would have been

§ *Ibid.*

in tension and would have been free from the minimum-depth requiremen imposed when they were designed as struts. About 5 feet of dead space the width of the bridge would also have been eliminated. No doubt th necessity of introducing light lateral bracing between the two arch ribs wou have found opposition on æsthetic grounds, but the design was one whic had great possibilities from the economic point of view. The simplici and absence of complication or expensive details was evident. A featu of particular interest in such spans, especially for railway loadings, wa that as a load advanced from one end, the point of contra-flexure or noo in the stiffening girder moved forward from about the centre of the spa ahead of the load. The natural frequencies of the two parts were ther

Figs. 29.



fore different and were constantly changing, and synchronous vibration were unable to build up.

With regard to the anchorage sections of the stiffening girders, th details were such as to suggest that the use of mild steel together with welded connexions would have led to considerable simplification in fabrication and a great reduction in size and weight. Had the possibilitie in that direction been considered ?

The specification for high-tensile steel used in the stiffening girde permitted a maximum carbon-content of 0·30 per cent., which was such as to produce a martensitic structure locally had welded connexions been employed. The bridge structure, however, was one in which pulsating or alternating stresses were not of commanding importance, and brought welding within reach, provided that the carbon-content of the high-tensile steel could be limited to about 0·13 per cent. Had the employment of welding for the mild-steel floor-details been considered ? The adoption of welding instead of riveting would have enabled considerable simplification and saving of material to be effected in structural details.

In connexion with the specification for the high-tensile steel mentioned

Appendix II (p. 424 §), it was interesting to note that from 0·30 to 60 per cent. of copper had been called for, and it would be instructive observe, as time went on, what the anti-corrosive value of the copper as in London. With regard to Table V (p. 426 §), recording certain sts carried out on high-tensile rivets, could the Authors give any figures mparing the frictional grip of those rivets with that of "rivet-steel" vets?

Mr. P. L. Pratley, of Montreal, observed that the reference to the nditions governing the width of roadway contained a series of sound rguments, but that those were then followed by one that was not so und, namely, that which dealt with the assumed difficulty of diverting affic. The result, however, seemed to be entirely independent of any of hose arguments, as the four lanes adopted would appear to have been ecided from other considerations. Generally speaking, for a city bridge, he width should be that which could serve the approach roads, without he need for traffic to re-adjust itself to fewer lanes, as that procedure ctually caused confusion and a local reduction in speed. It would appear rom Figs. 3, Plate 1 (facing p. 446 §), that at the Battersea end a tram-vay track was in existence on the approach-road (Queen's Road), but rom the Paper and from the detailed views of the bridge it was gathered hat there was no intention of extending the trackage across the structure. As the bridge was comparatively short, the decision as between four and ix lanes was probably dependent, as the Authors observed, on the present nd prospective capacity of the approach-roads and the probability of ther bridges being practicable in the future in case the traffic on those pproach roads should increase substantially, leading to their further widening.

There was no doubt that the suspension-bridge was the proper type of tructure for the site, and further that the self-anchored type was appro-priate to the situation and governing conditions. The consideration of the anchorages was the initial reason for adopting that type of structure, but the low clearances, the short and heavy characteristics of the span, and the comparatively heavy cables, all assisted in confirming the wisdom of the decision.

The question of loading was of considerable interest to him, but unfortunately the terms in which it was described were not very lucid to overseas members not accustomed to the Ministry of Transport require-ments. Local loading seemed to be quite normal for the type of heavy traffic concerned, except that possibly the impact-factor was more severe than was considered usual in North America, where 30 per cent. was a very common factor, even for floor-slabs, stringers, and cross beams. The loading on the main cables and trusses, while not fully understood, was taken as being somewhat on the heavy side also. Had a constant uniform

§ *Ibid.*

load been used for the computations involved in designing the cabl towers, and trusses, or did the reference to "loaded length under co sideration" mean that for different members or different sections, th uniform live load also varied? How was the axle-load of 48,600 lb. p girder treated, both in regard to its effect upon the suspenders and upo the cable-stress? Even for a condition such as might be met with c the Chelsea bridge, with crowds of people standing on the sidewalks ove looking the river, it was difficult to understand the justification for 84 l per square foot on 12 feet of each sidewalk and the full length of the bridg as a load contributing to the stress in the cables. Referring to the dea load, at first sight it would appear that that also was on the heavy sid but the information supplied was hardly sufficient for him to estima that weight with confidence. The buckle-plate system seemed to be ver popular in Great Britain, although no longer used in North Americ Was that because of a definite preference for something which had prove effective in the past, or was there some doubt about the permanency c economy or structural value of reinforced-concrete slab construction Granted that the heavy concentrations might call for considerable shea reinforcement, welded or slit-and-rolled trusses 4, 5, or even 6 inche deep had been used quite frequently in North America for such condition for example on the Philadelphia and Detroit suspension-bridges, on th Dorchester Street viaduct in Montreal, and quite recently on the Neche river in Texas. What method, if any, was adopted for the calculatio of relationships between the wheel-loads and the dimensions of the buckle plate and plain concrete slab as adopted for the Chelsea and so man other British bridge-floors?

The reference on p. 387 § to the temperature-stresses seemed to him to need a little further explanation to relieve it from ambiguity. Th facility to increase in scale uniformly in all directions in the two-dimen sional plane, without increase in total weight, just as though the elevatio were seen through a magnifying glass, was the condition leading to th absence of internal stress from temperature. It was not only the homo geneity of the material, but also the fact that that particular type o structure could be regarded as self-contained, and not restrained to earth that should be quoted as justification for the statement in the Paper Strictly speaking, the articulation-system tied the bottom of each towe to the comparatively inelastic piers, but, for the small temperature variations likely to occur, the total neglect of stress from that source wa perfectly justifiable, whilst the inquiry into the effects of uneven tempera ture as between cables and floor-system was commendable.

The other references to articulation and its effect upon stress-deter mination were equally interesting, and in fact he felt that, having said that much, the Authors might possibly have proffered even furthe

§ *Ibid.*

etail. The accuracy of the statement on p. 390 § to the effect that "the ertical couple at any section is dependent only upon the horizontal thrust n the stiffening girder and the vertical distance between stiffening girder nd cable," might be disputed in so far as there was bound to be moment n the girder itself under appropriate live-load conditions, except at the inge-points. On p. 397 § it was admitted that even dead load induced a egative bending moment in the centre girder span.

He felt that more information might have been given regarding the ables. The remark on p. 392 § regarding the use of No. 6 gauge wire in merica needed some elucidation. Whilst it was true that almost all wire or parallel-wire cables had of recent years been 0·192 inch in diameter, it vas not true that every effort was made to use that same gauge for stranded ables; that was to say, the effort was not made by designing engineers. That gauge was often specified as the maximum permissible, and as there vere some arguments which led to the belief that the maximum size and ninimum number of wires made for economy in the manufacture of cables, ope-making plants might, and occasionally did, seek permission to use hat maximum size. On the other hand, as the allowable working loads n tension tended to increase, designers rather liked to see the size of wire lecrease, so as to increase the length of line contact between the wires of ne strand and those of the strand immediately below or above it, where he strands passed over the saddles and loaded the tower with vertical omponents from the cable-pull. Serious compression-stresses between trands arose at those points, and the greater length of line contact per inear inch of cable served to reduce the intensity of that line compression nit. In the case of the Lions' Gate bridge at Vancouver, now under construction, Mr. Pratley, whilst admitting 0·196-inch-diameter wires galvanized) as the maximum, was pleased to note that most bidders for wire and rope offered strand-designs using smaller wires, many being about 0·165 inch diameter. As a matter of fact, however, wires of 0·196 inch diameter did actually constitute thirty-nine of the forty-seven wires in the accepted 19/13/7/7/1 design, but solely because American wire-makers were the successful bidders and that because they had just completed huge tonnages of 0·196-inch-diameter wire for the San Francisco bridges, and were in excellent position from the point of view of experience and equipment to bid low. Thus it was only the fact that a standardized wire-diameter, of which large tonnages were constantly being drawn, could be supplied at lower cost, that brought that size so prominently into the final make-up of cable-strands.

Then again, the Authors, in referring to the protection against corrosion offered by the stranded cable, omitted the most important feature, namely the heavy galvanizing. The "drawing" of galvanized wire, however desirable for precision-fitted strands, largely removed that protection, and

§ *Ibid.*

instead of the usual six-dip resistance obtained in Mr. Pratley's practic the wire in Table I (p. 394 §) seemed to have become de-galvanized aft two or three dips.

The mention of 2,000 feet as an approximate limit for stranded-cab suspension-spans was another very interesting point; was that figu the result of definite study independently made, or had it been obtain from some other source? Up to about 4 years ago, he was inclined place the limit somewhere between 1,500 and 1,600 feet for heavier span say with four lanes or more, and 1,800 feet for lighter spans. T criterion then had been, and probably still was, the development anchorage-details, and naturally span-length alone did not govern t matter. New systems of connecting strand-sockets to anchor-system had been developed in more recent years by Mr. Pratley's firm and other so that the limiting size of cable of that type had not yet been reache At Vancouver, sixty-one strands of $1\frac{7}{16}$ inch diameter for a 1,550-foo span carrying three lanes of traffic were being anchored without difficult and he had no reason to doubt the practicability of developing similar an satisfactory anchorage-details for an alternative span of 1,800 feet for t same site when tentative designs had been made about 2 years ago. T argument that wrapping a stranded cable at the Chelsea bridge und the conditions that there obtained would be difficult, was probably qui sound. Other means had been suggested in other places for protectin stranded cables between suspender-bands, such as sheathing them i sheet metal, welded after pulling tightly around the hexagonal or circula cable, but so far such means had not proved attractive to responsib designing engineers.

Generally speaking, the locked-coil type of strand as used at Cologn and Belgrade fitted the circumstances at Chelsea very well, although h would not regard it as having been demonstrated as more economic o more efficient than the wrapped cable of stranded construction usin circular wires.

Whilst it was recognized that further details of cable-properties an costs were not to be expected in a Paper of the type under discussion engineers closely in touch with suspension-bridge design and constructio were naturally interested in those matters, and were liable to be somewha disappointed that the data furnished did not permit full information t be compiled. From Table I (p. 394 §), the area of a strand (the aggregat of ninety wires) was 2·4968 square inches (virtually 2·5 square inches and the weight was listed as 9·25 lb. per foot, yielding a specific weigh of 3·56 lb. per foot, which was not unreasonable. The aggregate tensio at average values was shown as 211·56 tons, or 475,000 lb., yielding a average unit-tension value of 190,000 lb. per square inch. The only tes mentioned in the Paper was a strand which broke at 182·7 tons, o

§ *Ibid.*

10,000 lb. per square inch, an efficiency of only 86·2 per cent., which eemed much too low. He had just tested some of the strands for Lions' ate bridge, and had secured efficiencies as high as 97·1 per cent. on recisely the same basis, all those tests (twenty-four) giving values of over 6·9 per cent. The elastic modulus, as found during the pre-stressing of ne hundred and twenty-two strands 3,400 feet long, was very uniformly 4·7 $\times 10^6$ lb. per square inch, using the actual calipred diameters. The uthors noted that their locked-wire coils had a lower value of Young's odulus, but they did not quote the figure, neither could that value be found rom the information given in the Paper. Adopting a value of Young's odulus of 21 $\times 10^6$ lb. per square inch, the 15 inches of stretch under ead load given on p. 419 § would indicate about 36,000 lb. per square nch for the dead-load average unit tension on a strand 766 feet long, anging with a 40-foot sag in the 352-foot span. That in turn suggested very conservative design unit, but the live load being so indefinite to hose not acquainted with the standards referred to, that important unit was also unobtainable from the Paper. He was using 88,000 lb. per square inch as a working unit for wire with an average tensile strength of 220,000 lb. per square inch, every coil being tested, under loads described as dead, plus live loads, plus temperature-effects. Actually 90,000 lb. per square inch was used in computations, but the final figures for weights of the suspended structure and areas of cables yielded the former figure as the maximum stress likely to be reached under the assumed conditions of loading.

The cleaning of broomed wire previous to socketing had often proved a delicate matter, and many different solvents had been tried and found wanting. Obviously the nature of the impurity to be removed had a very important bearing on the choice of a solvent, but it would be interesting to learn whether an alkali or an acid solvent had been used at Chelsea, and whether the galvanizing was removed deliberately or incidentally during the cleaning process. The sockets were very conveniently designed for pouring, which in Mr. Pratley's experience was not always the case. The use of a white-metal alloy had often been pointed out to him as the standard British practice, whereas pure zinc was the universal medium in North America, as far as he had been able to establish. The advantage of pouring at 650° F. instead of 850° F. was real, but were there any other good reasons for not using spelter?

The tower-design, whilst unusual, was a logical development for the particular site, and no criticism should be levelled at the omission of sway bracing in the circumstances. The required ability of the towers to lean shorewards at various stages of erection, in conjunction with their inherent sturdiness, doubtless provided one of the reasons for the hinged footing; nevertheless the first impression received from an inspection of

§ *Ibid.*

the elevations of the bridge was that of a reproduction of the Mulhein structure, and the second was that those hinges constituted very needld like supports for an otherwise rather "solid" bridge. Had the use c flared-out rigid bases and temporarily-movable saddles been seriousl considered, and if so, was it found to involve any difficult situations or t be uneconomic? It was presumed that access to the insides of the tower was provided, with inspection-ladders enabling the maintenance-staff an the painting gangs to pass from section to section between the horizont and vertical diaphragms.

In regard to æsthetics, he had consistently argued in practice and i lecture for truth and therefore "functionalism" as the only basis upo which to found an opinion on the question. As "æsthesia" mean "feeling," and "feeling" was a question of emotion, the sense of fitnes both to perform duty and to harmonize with the surroundings was funda mental to true æsthetics. The only criticism that arose in his mind wa that the towers should carry some vertical lines on their surfaces seen i elevation, to emphasize the direction of their principal stresses, and tha they should not finish so abruptly at either top or bottom. Thus the pin joint offended the senses, which sought for suggestions of stability and con tinuity with the masonry piers, and the absence of some mass above th cable suggested a lack of finish in holding the latter down to the saddle an changing the direction of such a considerable portion of the cable stresses

Mr. H. Shirley Smith, of Calcutta, would be glad to know whethe towers fixed at the base instead of hinged had been considered. In hi opinion fixed towers would have added to the appearance, the economy and the rigidity of the bridge.

In regard to appearance, if the towers had been fixed at the base lateral loads could more satisfactorily have been resisted directly b holding-down bolts, and the two side bearings on each leg of the towe could have been omitted. That would have preserved an unbroken lin for the edge of the tower above and below deck, when viewed from th three-quarter position. Furthermore, the omission of the rocker bearing with its sudden constriction in the width of the tower would have preserved that same line when viewed directly in elevation.

From the economic point of view, what the Authors referred to as "a mass of metal" (p. 402 §) of a relatively expensive type would have been saved in the rocker bearings themselves, and an additional saving would have been made by omitting the side bearings. The improved rigidity resulting from a fixed base approximately 8 feet square above that of a system of three line-bearings was obvious.

There appeared to be little doubt that a fixed tower could deflec sufficiently at the top, in the final condition, without overstressing itsel in bending. The bending stress at the bottom of a 70-foot tower with

§ *Ibid.*

in 8-foot base would only be about $2\frac{1}{2}$ tons per square inch for 1 inch deflexion of the top. The Paper did not state what the deflexions were, nor the make-up of the tower, but a deflexion of that order would be expected. In erection it would be very simple to slide the tower-saddle over on top of the tower for a distance of 6 inches, or whatever was necessary to facilitate the connexion of the suspenders in the side spans to the main cable, and then to jack the saddle back to the central position to connect the suspenders in the main span. That was, he believed, the general practice in America. The extra material in the sliding bearing under the saddle would be negligible. He congratulated the Authors on seizing an opportunity that would probably seldom be presented of omitting portal bracing between the towers.

In any suspension-bridge, the weight of the stiffening truss depended ultimately on what live-load deflexion the designer was prepared to permit in the roadway. Generally, with live load of maximum intensity covering the right-hand half of the near truss and the left-hand half of the far truss, the maximum cross slope occurred in the roadway at the quarter-points. What did the maximum deflexion amount to there? Had any provision been needed in the roadway and hanger details for such deflexion?

What was the reason for using high-tensile steel rivets? He quite agreed that on a long span, the economy derived from the use of high-tensile steel was only fully realized if high-tensile-steel rivets were used. On a short span, however, where most connexions had "minimum" numbers of rivets, would it not have been more economical to have used ordinary mild-steel rivets? The Authors apparently did not fear electrolytic action between different kinds of steel setting up corrosion, as they had employed mild-steel diaphragms in the high-tensile-steel stiffening truss.

In his opinion, the Paper should have included a brief summary of the costs of different parts of the work, supplementing the few figures given on p. 420 §.

The Authors, in reply, observed that Mr. Maunsell questioned whether lock-coil ropes offered any advantages for suspension-bridges. They agreed that in suspension-bridge cables there was comparatively little flexure, movement and wear on the rope, but in cases where those effects were of primary importance, locked-coil ropes were generally constructed of a smaller number of larger wires than would be used for suspension-bridges. Ropes for the latter purpose were designed so that axial pressure was distributed over suitably-designed surfaces, and necking or crushing of the various layers was obviated, and a constant modulus of elasticity could be obtained; further, those ropes were cylindrical, so that a better nesting in the tower and other castings was assured. Those advantages,

§ *Ibid.*

in the opinion of the Authors, justified the extra expenditure, which incidentally was considerably less than the figure of £20 per ton mentioned. Further, the locked-coil rope, provided as it was with layers of accurately-drawn interlocking wires, gave security against the ingress of moisture, thus minimizing corrosion, which was bound to be considered an important factor. With regard to pre-stressing (the actual cost of which was also much less than the figure mentioned by Mr. Maunsell), the operation was undertaken with the two views of removing the initial stretch and of giving all ropes an equal and constant modulus of elasticity within the load-range, thus ensuring that all units of the rope would be equally stressed when under load. Each rope used on Chelsea bridge was pre-stressed with a load of 55 tons, which figure was somewhat higher than the actual maximum total of dead plus live loads plus temperature-effects which amounted to 53·8 tons working load. Mr. Banks and Dr. Nichol asked for further information on the modulus of elasticity of the ropes. Accurate readings to establish the modulus of elasticity were taken over a range of 20 tons, namely, between the dead working load of 35 tons and 55 tons. A value of approximately 20,000,000 lb. per square inch was obtained after the first reading, and on a second application of the loads (in other words, after pre-stressing), a value of 21,600,000 lb. per square inch was obtained. The permanent lengthening observed in the ropes amounted to approximately $2\frac{1}{2}$ inches between the limits of loading of 5 to 55 tons. No records were taken for loads under 5 tons. The incremental measurements of length were accurately read to within $\pm \frac{1}{32}$ inch.

In further reply to Mr. Banks, the actual reels used were 6 feet 3 inches diameter, compared with approximately 3 feet generally used for ordinary round stranded ropes. After reeling and unreeling several times, no appreciable shortening was found. Mr. Pratley raised the question of cleaning the broomed wires before their socketing. The cleaning of the broomed wires was accomplished without the use of acids or alkalis: the bitumen was removed by means of cloths soaked in paraffin, and the wires were finally cleaned with trichlorethylene and dry cloths, great care being taken to prevent the solvent from creeping into the adjacent part of the rope; the wires, having been galvanized and drawn, had a smooth surface which did not develop cracks, fissures or flaking when bent. None of the surface was disturbed when the ropes were being cleaned, and a good surface was available for adhesion of the capping metal. The galvanized surface was certainly not removed deliberately.

Mr. Banks and Mr. Pratley referred to the weakening of the galvanizing which took place during the "drawing" of the wires. It was true that the thickness of the covering would be reduced, but the action of drawing had the effect of forcing much of the zinc into the interstices on the outer surfaces of the steel, and the actual protection was therefore not weakened as much as the thinning of the covering would suggest. Mr. Pratley was under a misapprehension when he suggested that the wires became de-

galvanized during test after two or three dips. Actually, after that test, no deposit of bright metallic copper was observed which would not easily wipe off.

The weight of steelwork in the entire bridge, excluding the cables, was nearly 3,240 tons, the weight of the cables being approximately the figure of 330 tons that he mentioned.

Mr. Banks observed that hanger-rods had been used in preference to wire-rope hangers, and asked the reason for the provision for adjustment in those hangers. The coupling nuts certainly allowed an adjustment of the length of the hanger-rods if required, but the main reason for their insertion was to provide rods of convenient length for annealing.

In reply to Mr. Pratley, the mention of 2,000 feet as an approximate limit for stranded-cable suspension bridges was not the result of independent study.

Messrs. Banks, Pratley and Shirley Smith had commented on the adoption of rocker-towers. The towers were arranged on rocker-bases to provide for the movement of the tops of the towers for erection-purposes. The Authors took the view that in addition to the possible economy, and in some opinions, to the more æsthetic appearance, the simpler erection-procedure resulting from the use of rocker-towers justified their provision. Further, in reply to Mr. Pratley, access to the interior of the towers was provided, together with the necessary inspection-ladders.

Mr. Shirley Smith desired to know the reason for using high-tensile-steel rivets. It was considered advisable to cover fully all joints and, in order to keep the joint-covers within reasonable dimensions, high-tensile-steel rivets were used.

Dr. Nichols asked if the possibilities of welding had been considered in the anchorage-sections of the stiffening girders. The Authors were not at the time of the design in favour of welded connexions, but they agreed that had welding been permitted, it would have resulted in simplification of fabrication; they doubted, however, if any economy would have been effected by the use of mild steel with welded connexions. In regard to the comparative frictional grip of high-tensile- and mild-steel rivets, the Authors had not had tests made, but publications on the subject indicated that the frictional grip of high-tensile-steel rivets was not superior to that of ordinary carbon-steel rivets.

Mr. Pratley was interested in the question of loading, and he suggested that the description given in the Paper was not very lucid to overseas members of The Institution. The Authors were pleased to give more concise information on the subject. Uniform loads, to include impact, of 140 lb. per square foot of roadway and 84 lb. per square foot on the footways, together with a knife-edge load of 2,430 lb. per foot width of roadway, had been used in designing the cables and trusses. Those loads were 90 per cent. of those generally required by the Ministry of Transport for such bridges. The 22-ton axle-load which appeared in the Ministry of Trans-

port's standard train was allowed for in the equivalent loading curve, and thus did not enter into the cable-calculations, but it was used as a local load for determining the stresses in the hangers, cross girders, etc. The load of 84 lb. per square foot for footway loading was not considered high in Great Britain. Pigeaud's method, as developed by the Ministry of Transport, was adopted for the concrete-slab calculations for the wheel-loads. The calculations for the buckle-plates were based on Winkler's formulas, which the Authors believed was the usual American practice.

Dr. Nichols' question regarding the permissible stress in the stiffening girders, which were subjected to combined compression and bending, was of general importance in view of the difficulty sometimes experienced in determining what the permissible stress should be for compound influences. In the present case, however, the general theorems of elastic stability were easily interpreted and applied, since the lateral restraint afforded by the floor and the bottom laterals, and the vertical restraint of the tension in the suspenders, prevented independent transverse deflexions of the stiffening girders.

It thus followed that the appropriate slenderness-ratio was the length between the hinges divided by the radius of gyration about the horizontal axis through the centroid. That ratio allowed a much higher permissible stress than could be allowed were the girders freed of lateral restraints, because, in the latter case, as remarked by Dr. Nichols, the permissible stress would be 7·3 tons per square inch compression (from the secant formula) for high-tensile steel if the slenderness-ratio were 80.

Further, he had asked what allowance had been made for the structural details in assessing the stiffness of the stiffening girders. Consideration was given to the effect of those details in the design of the stiffening girders. It was estimated that they affected the stress in the stiffening girders to the extent of approximately 10 per cent. of the moment of inertia, and 14 per cent. of the area of cross section. Those estimated percentages were used in the design.

Dr. Nichols suggested that had the pins at the ends of the centre-span stiffening girder been lowered, the effect would have been an equalization of the stresses in the flanges. The Authors agreed that had such an adjustment been made, it would have tended to have had the effect suggested, but in large members of complex form the distribution of stress was—without an elaborate analysis—too uncertain to warrant the adoption of Dr. Nichols' suggestion.

With respect to Mr. Pratley's remark regarding the vertical couple referred to on p. 390 §, it might be stated that the idea the Authors wished to convey was that in the design of a self-anchored suspension-bridge, no recourse need be made to the deflexion theory at present used for the design of other types of suspension-bridges.

§ *Ibid.*

CORRESPONDENCE

ON PAPERS PUBLISHED IN FEBRUARY 1938 JOURNAL.

Paper No. 5081.

"Supplementary Notes on Flow Through Model Sluices." †

By Herbert Addison, M.Sc., Assoc. M. Inst. C.E.

Correspondence.

Mr. M. G. Ionides, of Amman, Transjordania, observed that, describing the "interference effect" as the percentage reduction in C_d following on the closure of some of the openings, the Author found that the general results agreed with those given in his former Paper.*

It would be recalled that some of those earlier results had been fitted into a formula which Mr. Ionides had proposed in a Paper presented to The Institution ‡. That Paper was concerned with free flow between piers, and described the deduction of a formula in which the coefficient of discharge (corresponding to C_d in the Paper under discussion), was given as a function of the "obstruction-ratio" r, where

$$r=\frac{\text{total area of waterway at obstruction}}{\text{area of waterway in downstream channel}}.$$

The type of flow dealt with in the present Paper was of a more complex nature, and the assumptions underlying the function quoted above would not hold there, but it seemed as if some modification of that function might be useful. As a test, Mr. Ionides had taken from *Figs. 10* (p. 69 §), a series of ratios of C_d for two and four openings, for different downstream depths, and had calculated the corresponding ratios from his own

† Journal Inst. C.E., vol. 8 (1937–38), p. 53 (February 1938).

* "The Flow of Water through Groups of Sluices: Experiments on Scale Models, with Particular Reference to the Effects of Mutual Interference." Inst. C.E. Selected Engineering Paper No. 105 (1931).

‡ Paper No. 5026, "Flow Between Piers: The Case of Small Loss of Head." *Abstract published in* Journal Inst. C.E., vol. 1 (1935–36), p. 525 (January 1936). [The MS. of this Paper may be seen in the Institution Library.—Sec. Inst. C.E.]

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

theoretically-derived function :—

$$K' = \sqrt{\frac{1}{3r^2 - 4r + 2}}$$

where r had the value given above and quoted by Mr. Addison on p. 71 §. The results were :—

Downstream depth : centimetres	8	10	12	16
" Interference-ratio " from values of C_d taken from *Figs. 10* (p. 69 §)	0·87	0·87	0·91	0·94
" Interference-ratio " calculated from values of K'	0·82	0·85	0·87	0·87

Those values seemed to be sufficiently similar in magnitude and trend to indicate a possibility that an application of the function that Mr. Ionides had proposed in his Paper * might prove to be useful in eliminating that most awkward feature, the variable coefficient C_d. If the relation of K' to r were studied (as, for example, in *Fig. 4* of Mr. Ionides's Paper *), it would be seen that the general variations of the coefficient C_d, as illustrated by Mr. Addison's new data, might be interpreted thereby.

Mr. Addison repeated in his present Paper that anything which increased the ratio r would tend to increase the interference-effect. In his own Paper * Mr. Ionides had commented on that statement, which for the type of formula used could not be strictly correct, for if the ratio became unity there would be no obstruction, and the coefficient would be bound to be unity ; yet as *Figs. 10* (p. 69 §) showed, the coefficient might rise at intermediate values of r to more than unity, and was therefore bound at some stage to decrease as the ratio increased. The difficulty could be overcome by re-arranging the formula so as to include the ratio r as a variable.

The Author, in reply, whilst considering that Mr. Ionides's comments were very helpful, was becoming more and more inclined to doubt whether the coefficients of discharge of groups of sluices could be expressed by simple formulas. Whereas the formula put forward in Mr. Ionides's Paper * involved only the " obstruction-ratio " r, the Author had found it necessary in his own formula † to show the separate effects of (1) the number of open vents ; (2) the width of each vent ; (3) the width of the unobstructed channel ; (4) the gate-opening ; and (5) the water-depth in the downstream channel. It was, moreover, now apparent from pp. 57 § and 61 § that still another variable might have to be taken into account, namely the rate of discharge.

§ *Ibid.*
* Footnote (‡), p. 447.
† Footnote (*), p. 447 : Formula (8), p. 23.

The Author would again like to emphasize the significance of Dr. H. E. urst's demonstration of the three types of flow (p. 55 §), namely free ow, transition flow, and submerged flow. It enabled investigators to e that only under submerged-flow conditions was there any reasonable rospect of finding an accurate formula, suited to various rates of dis-harge, for evaluating the coefficient of discharge. Nevertheless Mr. onides had done valuable work in showing the limits within which simple ormulas might have a rough validity: although his own formulas related riginally to the flow between piers under small loss of head, they enabled he interference-effects in Mr. Addison's experiments to be approximately alculated, in spite of the great difference in the conditions.

Mr. Addison should have made it clear that his comments on inter-erence-effects referred only to the experimental range that he had studied: e would now be inclined to add a further qualification, that the tendency or the interference-effect to increase as the obstruction-ratio increased vas only likely to be found in transition-flow and in submerged-flow con-litions. If the Author correctly understood the last part of Mr. Ionides's contribution, Mr. Ionides had in mind also the stage at which free flow occurred, with the formation of a standing wave and a consequent tendency for the coefficient of discharge to diminish as the downstream depth liminished. Mr. Addison thought it likely that within that range the ordinary free-flow formulas, which were independent of the downstream lepth, could more profitably be applied.

Paper No. 5098.

"Shearing Stresses in Gravity Dams." †

By Serge Leliavsky.

Correspondence.

Mr. H. F. Wilmot observed that it had long appeared to him that, however much engineers had regarded shear as being of the utmost importance, the vast majority had had—almost of necessity—to be content to rely on the calculation of the direct stresses in allowing for stresses in structures, owing to the amount of labour involved in the methods used for estimating shear. The adoption of the Author's method, whether arithmetically or graphically, led to a simplification of that problem to such an extent that it should become a standard method for the calculation of stress in every type of masonry structure.

With regard to the relative importance of direct, as opposed to shear,

§ *Ibid.*
† Journal Inst. C.E., vol. 8 (1937–38), p. 73 (February 1938).

stresses, the linear relationship $\tau_{cr}=A+B\eta_{cr}$ adopted by Prof. K. von Terzaghi and others appeared to be reasonable, at any rate so far as practical engineers were concerned, but the Author's guarded statement in which he deduced that critical shear stress without normal stress was always greater than normal stress without shear, was based on the somewhat arbitrary assumption that the value of B was the least value found by experiment, namely 0·727. He then deduced that in masonry and concrete, shearing stresses were more dangerous than tension. Surely that assumed that the critical shear stress was attained before the critical tensile stress in a given structure ? In other words, even supposing that the relationship given above were correct, there was a time-factor omitted. Hence it followed that that equation did not necessarily hold at a given moment, and, in fact, it might be untrue when applied at that, or any other, instant of time.

He would also draw attention to what appear to be two slight errors in illustrations: in *Fig. 5* (p. 83 §) $\left(\sigma+\frac{\partial\sigma}{\partial y}dy\right)dx$ should surely read $\left(\sigma+\frac{\delta\sigma}{\delta x}dx\right)dy$, and in *Fig. 6* (p. 84 §) $\tau_u dy$ should read $\tau_u\beta dy$.

The Author, in reply, thought that Mr. Wilmot had expressed very clearly the object of the Paper, which was intended to facilitate the calculation of shearing stresses, and, by those means, to allow such calculations being more frequently used in practical dam-designing than had been the case in the past. He wished, also, to thank Mr. Wilmot for having pointed out the slips in the illustrations to the Paper.

The time-factor, alluded to by Mr. Wilmot, had certainly to be taken into account in problems relating to the stability of structures. However, in comparing the relative importance of two types of stresses, namely shear and tension, all other conditions would naturally be assumed to be equal, and in that case the lowest (and not the highest, as might appear from Mr. Wilmot's remarks) critical stress would correspond to the most dangerous—and therefore most important—type. It should, of course, be made quite clear that the term "critical stress" was meant to refer here to the "breaking stress."

Viewed in that light, the fact that the critical value of pure shear appeared to be lower than that of pure tension would necessarily indicate that shear was more dangerous than tension, provided, of course, that the fact itself were taken as proved, which was another matter altogether.

§ Page numbers so marked refer to the Paper. (Footnote (†), p. 449.)—Sec. Inst. C.E.

CORRESPONDENCE

ON PAPERS PUBLISHED IN

MARCH 1938 JOURNAL.

Paper No. 5121.

"The Subsidence of a Rockfill Dam and the Remedial Measures employed at Eildon Reservoir, Australia." †

By Rupert Grenville Knight, M.C., M.C.E., M. Inst. C.E.

Correspondence.

Professor Sir Robert Chapman, of Adelaide, observed that the members of the Board of Inquiry, on their inspection of the dam after the subsidence in 1929, had been faced by a problem that required prompt action and decision. The dam, over $\frac{3}{5}$ mile long and more than 100 feet high from the bottom of the foundations for a length of more than 2,000 feet, the construction of which had cost over £1½ million, was in a condition that was giving great anxiety, and even alarm, to the people living in the valley below, as well as to the engineers in charge. Over a length of about 1,200 feet the rock-fill on the upstream face of the corewall had subsided, leaving the face of the corewall exposed for a maximum depth of 26 feet. Moreover, the corewall was obviously damaged. Vertical cracks showed at most of the expansion-joints, which were 50 feet apart, and at other places, some of which went right through the wall at the top, whilst a particularly dangerous-looking crack extending through to both sides and showing an opening 1 inch wide on the upstream face, where the corewall joined on to the thickened section adjacent to the spillway, was exposed by the removal of rock filling at that place. Some minor diagonal cracks were also visible. The most disturbing feature was, however, that in the centre of each of two long straight sections of the corewall there was a deflexion downstream of no less than 4 feet 8 inches. It was hardly to be wondered at that there were loud demands from certain quarters that the reservoir should be emptied. Regarding the wall as a cantilever held at the bottom, calculation showed that, under a horizontal pressure which varied as the depth, the maximum deflexion that could be expected at the top, without cracking of the concrete, was only something of the order of 4 or 5 inches, and such a large deflexion as that existing indicated the presence of

† Journal Inst. C.E., vol. 8 (1937–38), p. 111 (March 1938).

horizontal cracks, most probably near the bottom of the wall. Those cracks would be staunched by the clay on the upstream side, but they would mean that the corewall was entirely dependent for its stability upon the supporting power of the downstream fill. It was to be expected that the curved portions of the wall, in each case with a radius of 700 feet, would be stiffer against horizontal pressure than the straight wall, and those parts stood and even showed a small deflexion upstream. Between them the straight part of the wall acted as a flat slab fixed along the bottom and along the two vertical ends. Considering a horizontal strip of the straight portion of the wall near the top, the pressure from the upstream side would subject it to a positive bending moment in the middle and to a negative bending moment at each end. That negative bending moment was clearly shown at the junction of the corewall with the approach wall where the crack was distinctly due to tension on the upstream side and to compression on the downstream side. At the junctions of the straight section with the curved sections the corresponding negative bending moment would tend to reduce the radius of the curved portion of the wall, giving a deflexion upstream at the centre of the curve. The upstream deflexion at the curves might thus have not been due to any excess of pressure from the downstream filling, but might be regarded as having been a secondary effect due to the downstream deflexion of the straight portion of the wall, while the curved part, being as a whole more stable, had remained stationary.

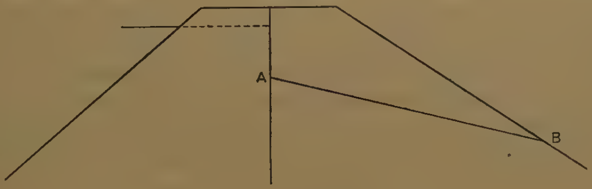
Bores put down through the rockfill disclosed, as was described in the Paper, that the clay wall on the upstream side of the corewall had in parts collapsed badly. At the point where the subsidence was greatest the top of the clay wall had sunk 51 feet, and bores at that section, after penetrating the clay, had encountered no more rock, so that apparently the pressure exerted by the clay had been such that it had actually forced the great mass of rock filling outward, the toe of the rockfill having been pushed outward at that point by no less than 55 feet. That movement of the rockfill had been facilitated by the fact that it rested, not upon the foundation rock, but upon a clay overburden, of various thicknesses of up to 20 feet, the surface of which, under the action of water, would be likely to provide a slippery layer on which such motion would become possible.

The behaviour of the clay wall seemed to point to two sources of weakness in the original design. In the first place the wall of clay, intended to ensure a tight dam, was entirely supported on the one side by the open rockfill, which allowed the water to penetrate it freely, so that when the reservoir was full the clay was in actual contact with the water on one side. In most cases where a clay corewall was used an effort was made, on the upstream side, to place a compacted material alongside the clay which would prevent a free access of water to the puddled wall, but that had not been done in the case of the Eildon dam. Secondly, the rockfill, which was normally placed on a bare rock foundation, had in that case been

laced on the natural surface of clayey ground that rested on the country ock some 20 feet below. That meant that when the ground was wet the tability of both the upstream and the downstream rockfill banks was eriously below what it would have been had the banks rested on rock. The effect of that had clearly been shown in the subsidence that took place vith the upstream fill, where the rockfill had been pushed bodily forward n a surface made slippery by immersion, and although no measurable novement had taken place in the rockfill on the downstream side of the orewall, the Inquiry Board had been concerned as to what might happen f the ground below the corewall were continuously saturated with water. The precautions taken to guard against that were fully described in the Paper. It was, however, one thing for such a Board to make decisions as o what should be done, and quite another thing to do what had been lecided upon; the task of carrying out the work had had to be shouldered by the officers of the State Rivers and Water Supply Commission, who were o be congratulated upon the way in which they had dealt with the difficult problems which they had been set.

Mr. E. V. Clark, of Adelaide, considered it surprising that the Eildon embankment, with its thin diaphragm corewall, should have been built

Fig. 30.



without consolidation of the fill, for that seemed to impart a great element of danger. If the corewall were considered as an impermeable membrane, without appreciable strength, then, if it were to remain vertical as built while the dam was empty, the static pressure of the fill on the two sides would have to be equal. The pressure per linear foot upon the top x feet was, according to Rankine's formula, $w\frac{x^2}{2}\left(\frac{1-\sin\phi}{1+\sin\phi}\right)$, where ϕ denoted the angle of repose of the material and w denoted its weight per cubic foot, subject to a slight correction for the limited width of the crest. As the reservoir filled, the pressure upon the upstream side increased, and hence for equilibrium the static pressure of the back fill had to be replaced by its dynamic pressure as the corewall deflected downstream. Following Rankine's theory, the maximum dynamic pressure that the backfill could exert down to any point A in *Fig. 30* was the force necessary to cause the superincumbent material to slide bodily down some line such as AB; evidently, however, that material could not be moved until it had

become considerably consolidated. It appeared, therefore, that it was only consolidation of the backfill which could prevent appreciable deflexion of a thin corewall when the reservoir was first filled, no matter how massive the downstream embankment might be.

As there was bound to be a certain amount of consolidation of the lower layers of a bank, due to the weight of the material above, the degree of consolidation was bound to be variable, and with a corewall strong enough to resist shear it would deflect as a cantilever, rigidly held at one end, and loaded unequally on the two sides. The fact that the Eildon corewall deflected considerably and irregularly appeared, therefore, to be amply explained by the lack of consolidation, without the necessity for postulating a pressure from the clay blanket equal to that of a fluid weighing from 70 to 100 lb. per cubic foot, and without blaming the wetness of the downstream foundations.

The Author stated (p. 146 §) that since the bank had been reconstructed the corewall had deflected further, but that a condition of stability appeared to be approaching. That also was as might be expected, since unconsolidated spoil might continue to settle for several years; further, as pointed out, above, additional material on the slope would not neutralize lack of consolidation against the core.

There had been built in Victoria at least one other embankment* with a diaphragm corewall blanketed with puddle, that had stood the test of time, and it would be of interest to know to what extent, if any, the corewall of that dam had become deflected since erection.

Mr. H. H. Dare, of Roseville, N.S.W., pointed out that the design of the rockfill section of Eildon dam was generally similar to that of an earlier structure built by the same authority on the Werribee river at Melton, Victoria. That dam, which stored water to a maximum depth of 98 feet, or 22 feet less than at Eildon, also had a central corewall of reinforced concrete, with a clay bank on its upstream side, and had been in successful operation since 1916. The central-core type had been used also, with satisfactory results, at Warwick, Queensland.

Modern practice favoured the different type referred to in the Paper, wherein the central core was replaced by an articulated reinforced slab on the upstream face of the rockfill, the whole weight of which then was used to resist water-pressure. A dam of that type, 70 feet high, was completed in 1936 for the Briseis mine in Tasmania, whilst a larger dam was under construction for the water-supply of Toowoomba, Queensland. Mention might also be made of the Manuherikia dam, 110 feet high and of similar construction, completed recently in New Zealand; in the United

§ Page numbers so marked refer to the Paper. (Journal Inst. C.E., vol. 8 (1937–38), p. 111 (March 1938).)—Sec. Inst. C.E.

* J. S. Dethridge, "Irrigation Works and Practice in Victoria." Trans. Inst. E. Aust., vol. 2 (1921), p. 93.

tates several dams had been built of that type, up to or exceeding 300 eet in height.

Reliance for water-tightness was placed either upon the central core-all, or upon the face-slab, but as an additional safeguard a clay bank was dded on the upstream side of the corewall at Melton and Eildon. At Ielton the thickness of that bank at its base was about 14½ feet, but at Eildon the thickness was made much greater, from 27 to 37 feet, by reason f the proposal to increase the height of the dam at a later date. That rovision, as it turned out, was unfortunate, for the great wall of clay, 0 feet high above the natural surface over the greater portion of the length f the dam (and, over the river-bed, considerably higher still), did not have he support from the full section of the upstream rockfill which it would ave received had the dam been completed to its proposed final height in ne operation.

In his Paper entitled " The Laws of a Mass of Clay under Pressure "[1] Ir. M. A. Ravenor stated that " the plastic stage is of interest in that it hould be avoided as far as possible." In selecting the clay to be used at Eildon the engineers had exercised proper care, and yet, as reported by the Board of Inquiry, " unfortunately when subjected to the continued action f water it appears to have become very plastic. . . . It is this particular eature in the behaviour of the clay used for the wall that seems to have been responsible for most of the troubles that have arisen."

In the early stages of the examination by the Board another suggestion egarding the cause of failure had been made; namely, that the sinking of he upstream bank, and the exposure of the corewall, might have been due o the fact that the natural clay and gravel formation underlying the bank had subsided. There was a depth of from 22 to 24 feet of overlying material on the flats, which was not removed before placing the bank (as was done at Melton); the material consisted of clay overlying gravel wash, with a thin layer of sand, but the view of the Board had been that, if the trouble had been caused by undue settlement there, there was bound to have been a spewing-up of the surface upstream, and of that there had been no evidence. The cause, therefore, appeared to lie in the failure of the clay bank, and that opinion had been confirmed later when the bores put down had revealed the surface to which the clay bank had slumped.

To remedy conditions in the upstream bank the Board had considered alternative methods, one of which had been simply to place more filling on top of the bank, thus squeezing down the clay until ultimately it took a stable shape. The other method had been to secure stability by placing additional filling on the slope or toe of the upstream bank, at the same time as material was added on the top, in the endeavour to prevent the clay from sinking down and spreading out further. The latter method had been adopted, and it appeared to have been successful.

[1] Minutes of Proceedings Inst. C.E., vol. 240 (1934–35, Part 2), p. 619.

A rather amazing feature was the extent to which the corewall ha deflected, while remaining reasonably tight. At the time of the Board inquiry the maximum deflexion at any point at the top of the corewall ha been 4 feet 8 inches, but that had increased since then to 7 feet 5 inche At Melton, where the corewall was only 1 foot 6 inches wide at the top an 2 feet 6 inches wide at the base (as compared with 2 feet and 6 feet a Eildon), the maximum deflexion had been about 19 inches at the top That smaller deflexion was due no doubt to the different manner of placin the downstream rockfill bank. The late Mr. R. H. Horsfield stated that at Melton the interstices in the broken rock were filled with scoria and that the mass was brought up in layers as the construction of the core wall proceeded, and was thoroughly consolidated by wheel traffic, as in a earth dam.

At Eildon the rockfill was deposited from side-tipping trucks, an suffered from the disadvantage of lack of consolidation, with the result tha the downstream bank did not offer adequate support to the corewall; o the other hand, when the upstream bank slumped, the downstream rockfi did not push the corewall upstream, as reasonably might have bee expected, and as the engineers were afraid that it might do.

To provide support to the corewall, and also to increase the friction resistance between the downstream bank and the natural surface belo the corewall, where bores had shown that the formation was water-logge the Board had recommended the addition of rockfill, as shown in Figs. 5 Plate 1 (facing p. 208 §). That additional weight had proved useful, n doubt, in checking any tendency for the bank to slide on the wet clay but the immediate effect upon the corewall had been unexpected, th deflexion at the worst point increasing by 25½ inches during the first 2 year after the settlement had taken place, and more slowly from then onward

It would be of interest to know what additions had been made to th downstream bank between November 1929, and May 1930, during whic period, as shown in *Figs. 12* (p. 137 §), the greatest increase in deflexio had taken place.

The behaviour of the corewall appeared to have been irregular, and no related to factors which might have been expected to have exercised a influence upon its deflexions. It seemed probable that the basic facto might have been settlement of the downstream fill, with telescopic re adjustment of the rock fragments of which it was built, but, whatever th cause, it was satisfactory to note that the wall had proved sufficientl flexible to remain practically impermeable in spite of its severe distortion The drainage-tunnel driven along the downstream base of the corewal had revealed less cracking than had been anticipated, and the tota leakage had been small.

* "Water Conservation and Irrigation Works in Victoria." Journal Inst. E Aust., vol. 7 (1935), p. 10.

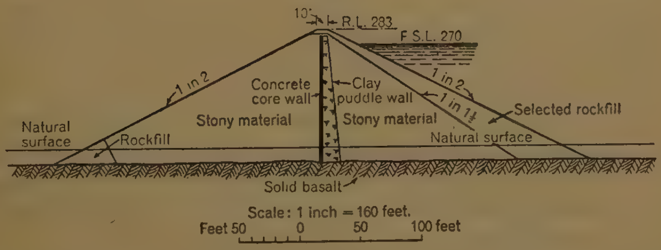
§ *Ibid.*

The reconstruction of the outlet-works had involved a considerable amount of under-water work by divers, to whom great credit was due. Some particulars might be given of the maximum pressures under which the divers worked, and whether there had been any cases of "bends." As reconstructed, the outlet-arrangements were a great improvement upon those originally provided.

The Goulburn catchment so far had not experienced any cyclonic storm, of the nature of that which had swept in from the sea and traversed the 5,000-square-mile catchment of the Murrumbidgee river, New South Wales, in May 1925, when it was estimated that the flood-discharge into Burrinjuck reservoir had exceeded 340,000 cusecs over a period of 9 hours. The Board, however, had considered that it would be wise to increase the spillway-provision at Eildon, and it was suggested that that might be done by the construction of an additional spillway on the Pinniger side. In view, however, of the not unjustifiable apprehension of the residents in the valley below the dam, an alternative scheme had been adopted, which gave better control of the flood-waters during the carrying out of remedial measures. Under that scheme the existing spillway-wall had been cut down over a portion of its length, and six gates, which had been constructed for later use in the Hume dam, had been installed, with the successful results described in the Paper.

Mr. L. R. East, of Melbourne, observed that the Author had given a detailed account of a dam-failure which was believed to be unique; the

Fig. 31.



design of the Eildon dam itself, however, was not unique, but had been based on that of a somewhat smaller dam constructed on the Werribee river, near Melton, Victoria. As no appreciable subsidence had occurred at the Melton dam, which was the prototype of the dam for the Eildon reservoir, a comparison of the two structures was of particular interest. It would be seen from *Fig. 31* that Melton dam was very similar to Eildon dam, and comprised a concrete corewall faced with a clay wall and supported on both sides by rockfill. In many other features, however, the two structures were dissimilar, and to those differences, no doubt, the Melton dam owed its comparative freedom from trouble. The more important of those differences appeared to be :—

(1) The Melton dam was founded directly on basalt rock in a comparatively narrow steep-sided gorge, whereas between the rockfill of the Eildon dam and the underlying rock was some 22 to 24 feet of clays, sands and gravels.

(2) The Melton dam, although termed a rockfill dam, was actually formed from the whole of the run from the spillway excavation on one side and from a quarry in partially decomposed basalt on the other, so that the bank contained a large proportion of fine materials, and might more properly be termed a "plum-pudding" dam. The resultant fill, although not watertight, was substantially heavier, volume for volume, than the light rockfill of Eildon, and probably weighed from 110 to 130 lb. per cubic foot, as compared with only 90 lb. per cubic foot for the material at Eildon.

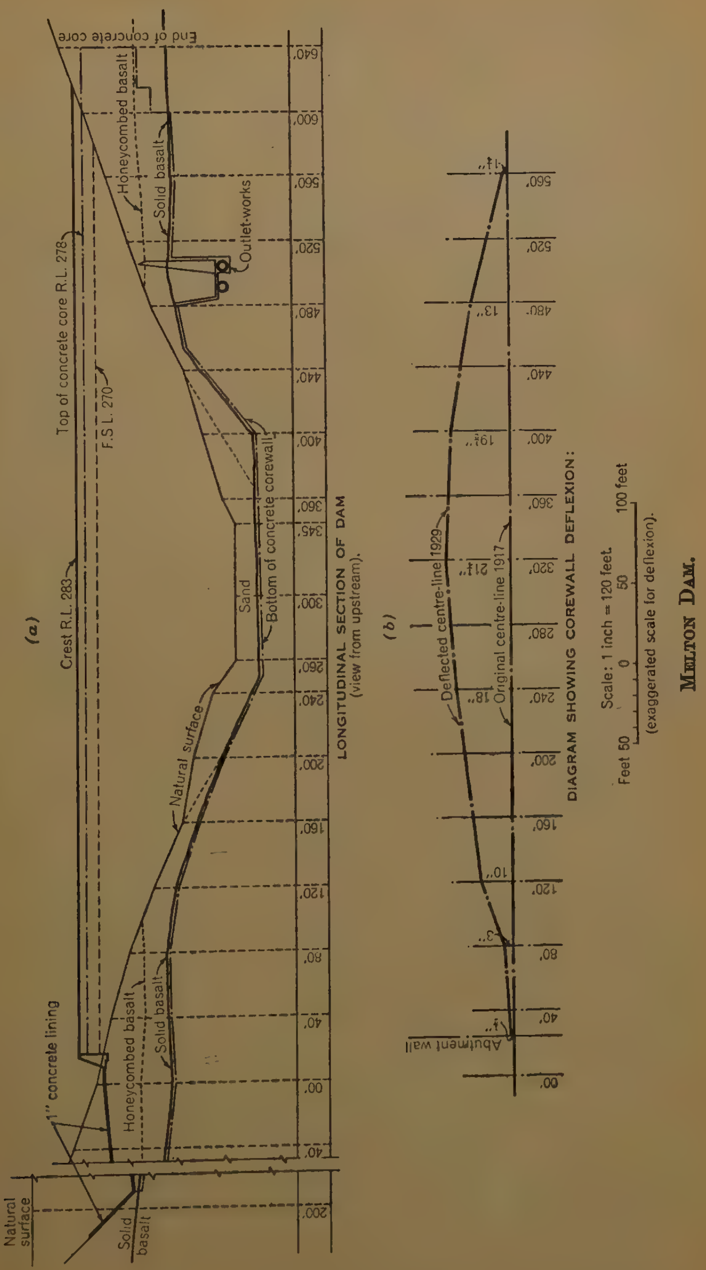
The clays, too, in front of the concrete corewalls would be dissimilar, but he knew of no comparative tests.

It was probable that the clay wall at Melton acted similarly to that at Eildon and exerted a considerable pressure against both the corewall and the rockfill, but that the Melton dam owed its continued security to a greater resistance against sliding offered by the upstream portion of the rockfill, and that that greater resistance was due to the two features emphasized above; namely, greater weight per cubic foot, and a hard foundation. Measurements along the crest of the corewall showed that deflexion had taken place, but not to the extent that had occurred at Eildon (*Figs. 32*). The maximum deflexion at Melton was 1 foot 9 inches in a height of corewall of 110 feet, whereas at Eildon it was 7 feet 5 inches in a height of 130 feet.

The Melton dam had been constructed in 3 years from 1914 to 1916 and had been very severely tested within a few weeks of completion, when a record flood had filled the reservoir in a single day to a height of 8 feet 3 inches over the full-supply level. When the dam had been drawn down during the following year, the upstream face was seen to be somewhat irregular, due obviously to differential consolidation or settlement of the "plum-pudding" fill of which the bank was composed. The structure had been carefully watched during the past 20 years, and no further movement had been observed. Although the Melton dam had proved satisfactory the failure at Eildon should be taken as a warning of the danger that was bound to accompany the use of large masses of clay in high embankments. It was probable that had the clay been omitted from the Eildon dam, no subsidence would have occurred.

The Author indicated that in his opinion a rockfill dam should be founded on sound clean rock, should be composed of durable rock in the body of the dam, and should have its watertight membrane on the upstream

Figs. 32.



Melton Dam.

slope. Those were also the conclusions of American engineers experienced in the design and construction of rockfill dams.*

Mr. J. D. Galloway, of San Francisco, limited his observations to certain features of the design. He had recently contributed a Paper entitled "The Design of Rock-Fill Dams" to the American Society of Civil Engineers †, and some of the points referred to were set forth in more detail in that Paper.

Foundation Material.—As described by the Author, the dam had been founded upon "the overlying material . . . composed of clay superimposed upon gravel wash," with the bedrock at a depth of from 22 feet to 24 feet below the surface. In addition to the material naturally in place, the material from the trench excavated for the corewall had been spread over the surface upon which the dam was to rest. It would appear that that procedure was unwise. Clay, especially when wet, formed a lubricating material upon which the imposed weight of the dam would tend to slide. As far as might be gathered from the description, the over-burden of the site should have been excavated to bedrock, as the material was not a proper one upon which to found a high dam. The presence of the clay in the material underlying the dam was probably an important factor in the horizontal movement of the upstream fill which, as shown in Figs. 9, Plate 1 (facing p. 208 §), amounted to about 55 feet.

Corewall.—The presence of a corewall in any dam was believed to be an error in design. Superficially it would seem to be the proper place for the impervious element, but that was only so when other important factors were ignored. The major forces involved in a dam holding water were made up of the weight of the dam, which was vertical, and the weight of the water, which developed forces in lateral directions. In the case of a vertical cut-off wall the water-pressure was horizontal. The resultant of those two forces brought into play forces that acted upon a cut-off wall differently from those which were developed by gravity alone.

The settlement of the rockfill on both sides of the cut-off wall was a variable that had to be taken into account in designing such a cut-off wall. The settlement was rarely directly vertical, as it occurred from the flanks of the dam towards the centre as well as vertically, the amount depending upon the slope of the canyon-walls. The magnitude of the forces acting against a vertical wall by a rockfill placed against it was uncertain. With earth fills there was sufficient data to enable an engineer to design a retaining wall, but when the fill was made up of large rocks the lateral forces developed against the vertical wall were, to a certain extent, unknown. When the rockfill had just been placed, the lateral forces were probably small, but after settlement the forces tended to increase. Those statements referred with greater effect to the upper portion of the dam than to the lower portions.

* J. D. Galloway, "The Design of Rock-Fill Dams." Proc. Am. Soc. C.E., vol. 63 (1937), p. 1451. (October 1937, Part 1.)

† Footnote (*), above.

§ *Ibid.*

As soon as the water in the reservoir reached the top of the cut-off wall the maximum downstream lateral forces were developed, and that usually took place before the rockfill had developed a corresponding resistance. The common result was a lateral movement of the rockfill downstream. That effect took place in all rockfill dams, irrespective of the position of the impervious element. Some data in regard to existing dams were given in Mr. Galloway's Paper.[1] The result of the settlement under lateral and vertical forces was to move the crest downstream, the corewall, if one be used, being ruptured and moving with the mass. In one dam of about the same height as the Eildon dam, with a corewall in the interior, the lateral movement downstream reached a maximum of about 13 feet.

Evidence of the lack of effective resistance in the downstream rockfill might be inferred from the fact that support for the corewall on the upstream side was removed by the settlement of the mass of clay and rock to a maximum of 26 feet without a rupture of the corewall, or without the wall being forced upstream towards its original position.

A second serious objection to the central corewall was found in the fact that all the material on the upstream side of the dam was reduced in weight by being immersed in the water, and its resistance to movement was correspondingly reduced. In effect, the only portion of the dam that resisted the water was that part downstream from the cut-off wall. The resistance of the dam against sliding might be reduced by from 30 to 40 per cent. in that manner, the amount depending upon the ratio of the sections of the rockfills.

Another objection to the corewall that might be mentioned was that, whilst it was the only defence against the water, it was inaccessible and difficult to repair.

Most or all of those objections found support in the behaviour of the corewall of the Eildon dam, which acted as others had done when built in the manner described.

Clay Fill.—The clay fill that was placed against the vertical cut-off wall was a feature that he had never met in other dams, although there might have been precedents based upon experience in earth dams. It was believed that the presence of the clay fill had caused much of the trouble at the Eildon dam, and might cause more in the future.

The clay seemed unnecessary. Modern concrete, mixed in a manner to obtain the greatest density, would resist water-pressures considerably greater than that at the Eildon dam. Dams in California with a facing of concrete 3 feet thick had successfully resisted pressures due to heads of up to 300 feet.

The Author had clearly explained the movements of the clay fill and the part it had played in the lateral movement of the upstream rockfill

[1] Footnote (*), p. 460.

and the sinking of the crest. There was little doubt but that the clay in the foundations and in the excavated material from the trench of the cut-off wall had assisted in the movement. A California earth dam had been badly ruptured by movements of the foundation-material, which included a stratum of clay. However, Mr. Galloway did not share the opinion of the Author that piling rockfill on the top of the clay was an active agent in causing the movement of the mass. The clay weighed about the same per cubic foot as the rockfill, and at the beginning there was but 5 feet of rock on top of the clay.

The clay fill, with an almost vertical face, was inherently unstable as built. At a height of 100 feet, horizontal pressures of 10,000 lb. per square foot were developed. Under such pressures clay would flow. When lubricated by water, movement might be expected in an increased amount. Table VIII (p. 127 §) showed that an increase of moisture-content of the clay at Eildon dam from 30 per cent. to 40 per cent. reduced the shearing value of the clay from 820 lb. per square foot to 333 lb. per square foot, a reduction of nearly 60 per cent.

It might be claimed that the clay mass would be sustained in position by the rockfill placed adjacent to the upstream side. There were, however, several reasons why that effect would not be sufficient to maintain the clay in position with a surface practically vertical. The first was that the amount of effective horizontal pressure of the rock was uncertain, and could only be materially developed by a certain amount of movement, which could not take place. The principle was the same as that discussed in connexion with the corewall. For that reason, movement of the clay mass might be expected, especially at the base where the horizontal pressures, developed by the weight, were a maximum. The second reason why the clay would not be held in place was that, in any movement of the clay, the resistance of the rockfill would not be effective because the clay would be squeezed into the voids of the rockfill, which might be of the order of from 25 to 30 per cent. of the mass of the rock. The tendency of the clay to move horizontally would thus meet with insufficient resistance. The movement of the clay would also be caused by the failure of the corewall to furnish resistance on the plane of contact between itself and the clay wall. Movement of the corewall amounting to several feet at the top would be followed by a corresponding movement of the clay.

It was not possible to differentiate between the effects of the several causes of movement in the Eildon dam. The lack of resistance to sliding in the material of the foundations, the inherent instability of the clay fill, the lack of effective resistance in the supporting upstream rockfill, and in the corewall on the downstream side, and the tendency of the clay to be forced into the voids in the rock, were all factors that, to a greater or lesser

§ *Ibid.*

degree, caused the movement that resulted in the deformation of the upstream part of the dam.

Whether or not the addition of the rock-fill made in the reconstruction of the dam would be effective might be questioned, although it was the only thing that could be done. The tendency to slide, due to the clay in the foundation-material, still existed. The mass of clay in the deformed position shown in Figs. 9, Plate 1 (facing p. 208 §), might be forced farther into the voids of the rock, with a resulting tendency to movement. The presence of the clay would remain a disturbing factor, and stability would only be reached when no farther settlement took place. It would seem that the clay built into the dam had been a major cause in the dislocation of the dam.

The points discussed had been developed by the Author, who had covered the essential elements of the problem that the engineers had had to meet in a clear and concise manner. He had also described what Mr. Galloway deemed to be the proper design of a stable rockfill dam, where the impervious element of the dam was placed on the upstream slope of the rockfill. The Author had also mentioned the laminated face of the San Gabriel No. 2 dam in California. For the information of those interested, it might be stated that that dam had developed an excessive settlement, and the laminated facing of concrete had been so badly ruptured that a temporary facing of wood had been installed. What would be done with that dam as a permanent facing had not yet been determined. Mr. Galloway believed that it was a mistake to use a laminated concrete facing for a dam, because it would tend to split apart under the various stresses to which it would be subjected when in service.

Mr. A. E. Kelso, of Melbourne, thought that the total measured leakage through the restored structure was to be considered moderate in view of the severe damage to which it had been subjected. One aspect of the repair, however, left a doubt concerning the future behaviour of the structure. In some of the major cracks the reinforcing steel had been left exposed. At other cracks a repair had been made at the face of the wall, but should hair-cracks open in the sealing mortar water would have access to the steel at those cracks also. There could be no doubt that in many reinforced-concrete structures water had access to the steel through fine cracks. Under some circumstances alkaline conditions inside the concrete might retard or prevent corrosion, but there were many recorded instances where corrosion of reinforcing steel had been rapid, and it would seem that the comparatively great width of cracks in the concrete at Eildon might tend to that result. It seemed probable that the steel was already heavily stressed in some sections, and the weakening of it by corrosion might lead to failure of the steel, and to renewed movement of the wall.

The subsidence at Eildon was primarily a clay failure, and to those

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§ *Ibid.*

interested in the use of clays in similar circumstances it would be of great value to have the means of recognizing and avoiding clays of similar properties. The establishment experimentally of the shearing coefficients (pp. 126 § and 127 §) was of definite value in that respect, and the "ball," "sausage," and "saucer" tests had also some value; he felt some regret, however, that description and identification of the material had not been made by the methods of mechanical analysis—determination of particle-size and shape, and quantitative measurement of permeability—as commonly used in the recent developments of the science of soil-mechanics.

In the construction in 1925 of the O'Shannassy dam for Melbourne water-supply, he had had occasion to investigate the suitability of several classes of clay for a purpose very similar to that to which the Eildon clay had been put; namely, as a blanket to staunch the expected shrinkage cracks in a thin concrete corewall of an earthfill dam about 113 feet high. The O'Shannassy clay ranged in thickness from 4 feet at (or near) the crest-level to about 12 feet at the natural surface. The tests applied were the "ball" and "sausage" tests as described by Mr. P. a'M. Parker in his book, "The Control of Water" *, and substantially the same as those mentioned by the Author as having been performed at Eildon, and a percolation-test similar in its essential features to that carried out in the more modern variable-head permeameter equipment. The results of the percolation-tests were consistent in themselves and Mr. Kelso believed that test to be a reliable measure of that particular quality. The "sausage" test was found to be very sensitive to water-content and to the degree of preliminary manipulation of the material, but when those were reasonably controlled, results were found to be consistent. That was not so with the "ball" test. The durability of the ball in water appeared to be far more dependent on such factors as the actual volume of the testing water and the proximity of the ball to the sides of the vessel, than on the nature of the clay itself. In one test, five balls had been made from the same mass of clay, one ball having been placed in 50 times its volume of water, and the other four together in a similar vessel holding the same volume of water. The single ball had broken down in about 50 hours, but no one of the group of four had more than chipped slightly in 30 days. All subsequent tests had been carefully controlled in that respect (a volume of water equal to 30 times the volume of clay having been used), but even then results had been far from consistent.

As a matter of interest, the approximate average results of the O'Shannassy tests were set out in Table XIII, for comparison with the figures for the Eildon clay and the standard suggested by Mr. Parker as representing good English puddle clay:—

§ *Ibid.*
* London, 1913.

TABLE XIII.

Origin.	Description.	Percentage of "clay."	"Ball" test: hours.	"Sausage" test: inches.	Percolation test: inches in 30 days.
ildon . .	Yellow puddle from corewall.	70 to 80	24	15	—
'Shannassy	Red earth fill	35	5 to 10	14	1·2
,,	Selected earth fill	more than 35	20	39	0·6
,,	Decomposed dacite	—	40 to 60	48	0·4
,,	Red puddle clay		60 to 100	50	0·1
,,	Grey puddle clay	80 approx.	60 to 90	48	0·05
,,	Dark brown puddle clay		over 100	72	Nil
arker . .	Standard	—	48	10 to 12	—

In the third column the term "clay" as applied to the O'Shannassy gures meant the percentage of particles of size less than 0·005 millimetre U.S. Bureau of Soils Standard). Where the Author had used the term clay" it was presumed that it referred to that standard. The clays of he O'Shannassy group had all been subjected to the same controlled echnique, and the results were therefore fairly comparable. It was nlikely that the figures relating to the Eildon clay and those of Mr. Parker's standard were comparable with them for the reasons given above: f they were, however, neither the Eildon clay nor the O'Shannassy earth-ill clays came up to Mr. Parker's standard for puddle clays, whereas the D'Shannassy puddle clays (which had been used in the dam, and which o the present date had given satisfactory service) were superior to that standard.

Mr. Kelso considered that those responsible for the work described n the Paper should be congratulated on a very thorough repair, and the Author on a clear and interesting record of it.

Mr. A. D. Lewis, of Pretoria, in the course of an inspection of irrigation-works in Australia in 1935, had visited Eildon dam, and in his Report[1] he had criticised the design on much the same lines as those followed indirectly by the Author in his Paper. There appeared to be four major defects in the original design. Each of those defects introduced an element of weakness, and the combined effect might even be described as being very dangerous :—

(1) The rockfill should not have been placed on the soft surface, which consisted largely of clay. That soft material should have been removed and the rockfill should have been founded on rock.

[1] "Irrigation in Australia." Pretoria, 1935.

(2) The concrete cut-off wall should have been placed on the upstream face and not centrally, for reasons of economy and safety.

(3) Having adopted the central position of the corewall, the steep clay bank against the upstream face of the corewall should have been left out, especially as the rockfill was founded on clay.

(4) As a matter of specification and execution rather than of design it was questionable whether a large proportion of the material employed as rockfill was not liable to rapid disintegration.

It should be pointed out that the features criticized under (2) and (3) had previously been embodied in the Melton rockfill dam of the Werribee scheme, which was about 100 feet high.[1] A central corewall had been adopted with a steep clay face on the waterside, the latter being steeper than, and less than half the thickness of, the clay face at Eildon. No slumping or excessive settlement had occurred in that work, and the maximum deflexion of the central corewall had only been 19 inches. The rockfill, however, had been placed on solid rock and the material of the rockfill was not liable to disintegration. It might be surmised, therefore, that the major defect at Eildon was the first of those referred to above, and that the other features had been contributory to the failure.

In discussing the causes of the slump, reference should be made to the theories of sliding on cylindrical surfaces, as developed by Petterson and others, and more recently by Messrs. Fellenius, Iwanow, and several other writers at the Second World Congress on Large Dams in 1936. Those theories were based on observations of actual slumps. In 1937 the downstream face of the Bon Accord earthen dam in South Africa had slumped along a cylindrical surface. The material of the dam and the base on which it had been built were very clayey. During the course of the removal of the material in the slide the main cylindrical base of the slide could easily be traced by the high metallic lustre of both surfaces of contact, and it was found to have a radius of about 90 feet. The slide base was very steep in the made bank at the top, and gradually the surface curved round to the horizontal in the underlying natural clay, and then turned upwards to emerge near the toe of the bank. In the case of Eildon, if the clay against the corewall and below the rockfill were regarded as being continuous, it was possible, owing to the great thickness of the base of the former, to draw a circle of dangerous slip with a radius of about 90 feet, beginning almost vertically in the clay face, continuing entirely within the clay of the face and the base, and emerging through the toe of the rockfill. That theory involved the assumption that the slip was not in a straight line along the junction of the rockfill with the clay surface, but was curved within the clay base, and

[1] Footnote (1), p. 465.

would result in the pushing out of some of the clay at the upstream
be of the rockfill, which seemed likely from the fact that the slope of the umped toe, as given in Figs. 9, Plate 1 (facing p. 208 §), was much flatter han the angle of repose of rockfill. Once a slide of that sort had taken lace, the cylindrical surface was for ever a source of weakness. In fact, would appear from the remarks on p. 128 § that there had been some idings prior to those of 1929, possibly during the draw-off of 1927, when he water had been lowered more rapidly and to a level some 60 feet lower han in 1929. Towards the end of 1927, as the water was down to a level f about R.L. 713, it was bound to have been confined within the river-hannel, and practically the whole of the dam and the original surface at he toe should have been visible. Was there no information available as o the appearance of the dam and the ground-surface about that date, nd had there then been no warning of impending trouble ?

In regard to the material used for the rockfill, on p. 118 § the Sugarloaf ock was described as being more durable and as resisting weathering to greater extent than the Pinniger mudstone, from which it would be oncluded that the Pinniger mudstone was defective in those respects. Photograph No. 7 (p. 71) of Mr. Lewis's Report * showed some of that nudstone in the bank as having almost completely crumbled by weathering nd disintegration. If that material formed a large proportion of the ank, and especially if it were unequally distributed, it was possible that in he course of continuous crumbling dangerous strains would result in the orewall, which had very light reinforcement exposed to corrosion in the lready numerous cracks. Any failure of the corewall from that cause or rom further slides might result in the release of some 300,000 acre-feet of water, which might be rapid and disastrous in view of the ease with which the soft material of the base of the dam and the crumbling material in the rockfill could be washed away.

Taking into account all the elements of weakness in the dam, it was difficult to escape the conclusion that, in spite of the very sound recommendations for restoration and the very thorough manner in which they had been carried out by the Author, the work could not be considered as being free from the possibility of disaster. If there were such a possibility, then any consideration of an enlargement of the dam would probably be out of the question. It would appear that there was a demand for additional storage and that the run-off of the river would justify a storage at least three times as big as the present storage. Would it not be desirable to proceed with such a new scheme of storage at the earliest date at a new site, if such could be found ? It was understood that the whole cost of the original work and the restoration had exceeded £1,800,000, the unit cost having been therefore at the rate of over £6 per acre-foot. If new

§ *Ibid.*
* Footnote (1), p. 465.

storage for 1,000,000 acre-feet could be achieved at a new site, or sites, for less than £4,000,000, including all appurtenances such as the power-house, the unit cost of the old and new work combined would still not exceed £6 per acre-foot, even if the present storage were neglected on the assumption that the present work was put out of use.

A most interesting feature revealed by the measurements of the deflexion of the corewall was the fact that the corewall suffered no deflexion in the two regions of curvature, even though the radius was as big as 700 feet. In view of that, was it not desirable to introduce curvature into the plan of rockfill dams as an additional precaution ?

Mr. David Lloyd observed that from Table II (p. 115 §) it appeared that the average general rainfall, from a cartographical method, was about 45·6 inches per annum (although a value of 40 inches appeared on p. 114 §, for which there was probably a reason) over the catchment of 980,000 acres. The mean run-off, from Table I (p. 115 §), was approximately 19 inches per annum. The difference represented loss by evaporation, etc. There was, however, no reason why the rate of evaporation for given weather conditions should be different in Australia and in England. In order to investigate that, it would be of value if the Author could give the mean temperature over the Eildon catchment. As a rough approximation Mr. Lloyd found in a reference atlas that the mean temperature over the catchment was from 54° F. to 56° F. The geological formation of the area appeared to be of Silurian age. From that data, utilizing a nomogram published recently *, the loss expected would be :—(*a*) rainfall at 48° F.: 15·8 inches ; (*b*) an additional 8·9 inches on account of a temperature 8° F. higher ; (*c*) an additional 1 inch on account of ground-water loss. That estimate came to a total of 25·7 inches, as compared with the actual figure of 26·6 inches. The closeness of the estimate might be fortuitous, but that it was so close was of interest.

Mr. James Mitchell observed, with reference to those sections of the dam at chainage 1,550 marked in Figs. 5, Plate 1 (facing p. 208 §), " as constructed " and " after subsidence," that the support of a wall 90 feet high and built of clay which had been selected on account of its plasticity, would under any circumstances be a somewhat difficult problem. When the wall was inside a reservoir, and was exposed to the continual softening influence of water having a depth, when the reservoir was full, of about 80 feet, and when the support took the form of a rubble mound resting on a bed of loamy clay, which also was exposed to the softening effect of the water, the difficulty was greatly intensified. The weight of the clay wall would impose a load on its bottom layer of about 5 tons per square foot, and to that would be added the downward drag of a wedge of rubble, the

§ *Ibid.*

* D. Lloyd, " Evaporation Over Catchment Areas." Quarterly Journal Roy. Meteor. Soc., vol. 64 (1938), p. 423.

ffect of which would be intensified by the smooth surface of the corewall, ith, in addition, the possibility of lubrication, owing to moisture in the yer of clay next to the concrete.

Reference was made on p. 130 § to "pressure of fluidity" as being a roperty possessed by clay, and as affording an explanation of the state f affairs shown by the section of the dam marked "after subsidence." here was nothing to be gained, however, by regarding clay as a fluid. t was a plastic material which, like lead, would yield indefinitely when xposed to an unbalanced pressure of sufficient intensity. It seemed robable that, owing to slight irregularities in the character of the clay, nd in the method of depositing it, the degree of softening, and therefore he lateral pressure produced thereby, would not be uniformly distributed. n addition, owing to the small size of the stones (described on p. 118 § s of "one-man" size), the rubble mass would be somewhat unsuited to esist a pressure which was unevenly distributed, whilst any movement ould at once greatly reduce its internal friction, and would thus further iminish its power of resistance.

On p. 128 §, and elsewhere, the rubble mound was represented as eveloping "passive resistance," whilst on p. 139 § it was described as xerting "active pressure." The expression "passive resistance," although n common use, was a contradiction in terms, and was apt to be misleading. Action and reaction being equal and opposite, all resistance was active.

Mr. W. H. R. Nimmo, of Brisbane, pointed out that the failure by slumping of several hydraulic-fill dams had clearly demonstrated the disastrous results which might follow from the inclusion of a great thickness of extremely fine or clayey material in the centre of an earthen dam, and the necessity for careful control of the grading of the filling placed in such a structure. In the case of the Eildon dam, the core, which consisted of a clay pug without the inclusion of coarser material to stabilize it, comprised a considerable proportion of the width of that portion of the dam which was upstream of the concrete corewall. The failure of the dam had confirmed, if such confirmation be required, the experience with some other structures. In the case of the hydraulic-fill dams, the water needed to saturate the clay was introduced during construction, whereas in the Eildon dam the core had been permeated by stored water after filling of the reservoir. Having regard to the nature of the natural material upon which the eastern portion of the dam had been built, the drainage-system provided was inadequate.

Control of outlet-conduits at their upstream end had proved to be thoroughly unsatisfactory. When, as at Eildon dam, the upstream control was to be effected by sliding gates, subjected to a head exceeding 100 feet, and intended to function at part openings, it was not surprising if serious trouble were experienced in operating the outlets. As the Author showed,

§ *Ibid.*

conditions had been greatly improved by the application of downstream control, but the necessity for retaining the existing 4-foot 6-inch diameter pipes imposed restrictions which had prevented the designer from obtaining the best conditions in the remodelled outlets. It was understood that each needle valve was capable of discharging 1,300 cusecs under a net head of 50 feet at the valve. Owing to the 6-foot 6-inch pipe having a greater area than the outlet of the valve, about half the head on the latter would be static head, and therefore operating conditions should be satisfactory even at full opening of the valve. Since the area of the 6-foot 6-inch diameter pipe appreciably exceeded the combined area of the two 4-foot 6-inch diameter pipes, kinetic energy equivalent to about 300 h.p. had to be converted to static energy or to be dissipated within the breeches pipe, the shape of which was such that the conversion was likely to be imperfect. Dissipation of energy probably accounted for the detonations reported by the Author at 0·35 opening of the valve, but the disturbance might be accentuated at full opening. Without emptying the reservoir it would not have been practicable to have increased the size of the 4-foot 6-inch diameter pipes, but a decrease in the size of the 6-foot 6-inch pipes might have improved conditions.

The spillways of Australian dams had frequently been designed to cope with a peak flow somewhat in excess of that of the largest flood recorded in the particular stream. Large dams, especially earthen or rockfill dams, should be provided with spillways which would safely pass a 1,000-year flood, but owing to the fact that streamflow records were usually available for recent years only, it was extremely unlikely that the greatest observed flood even approached the magnitude of a 100-year flood, and consequently several dams in Australia had insufficient spillway-capacity. If sufficient records were available on neighbouring streams having comparable catchment-areas, a probability investigation should be made to determine the probable peak flow of the 1,000-year flood, or, failing that, the spillway-capacity should be based upon the maximum flow recorded from areas of similar size and situated in similar climates to that of the stream in question. For a catchment-area of 1,500 square miles of the type of that of the upper Goulburn river, a peak flow exceeding 100 cusecs per square mile was possible, and precipitation continuing for 2 or 3 days at the rate of 6 inches per day might, if falling upon a saturated area, produce such a run-off. It appeared, therefore, that, even allowing for the ponding effect of the reservoir, the spillway-capacity at present provided at Eildon dam could not be regarded as being excessive.

Mr. E. G. Ritchie, of Melbourne, observed that there were many valuable lessons to be learnt from the subsidence of the dam, not the least of which was the danger of reducing profiles in a dam composed of loose rockfill. The Author stated on p. 131 § that the fill weighed approxi

§ *Ibid.*

ately 90 lb. per cubic foot, as compared with 120 lb. per cubic foot for nsolidated earth. The value of 90 lb. per cubic foot was high for loose ck, after making allowance for 50 per cent. of voids, and if the Eildon ckfill reached that figure in all parts of the fill, it was bound to be regarded an optimum expectation. Many loose rockfills would weigh less, and e submergence in water of the upstream fill would further reduce the eight.

That was the fundamental error in the design, in view of the great uid pressure exerted by the clay wall, which had evidently not been fore-en. Calculations showed that, with such a rockfill, founded on a slippery ay base, the margins against sliding, both of upstream and downstream l, were very small, if opposed to fluid pressure from the clay. Most ckfill dams had been founded on a rock base, and the dangers which had ecome manifest at Eildon dam would not be present, owing to the greater iction on the base.

The Inquiry Board, in dealing with proposed remedial measures, had een faced with a dilemma. The first proposal had been that additional ckfill could be placed on top of the subsiding fill close to the corewall on s upstream side, so as to protect the concrete wall against calculated angers of overturning from the downstream fill; in that connexion the uthor's comments on pp. 138 § *et seq* were of special interest. The ffect of continuing to deposit additional fill at that part of the dam would ave been gradually to compress the clay wall beneath and to force it into shape where it would no longer be a menace in pushing the rockfill out nto the water. It had to be remembered, moreover, that the reservoir ould not be emptied, for, as soon as the water had been lowered beyond the ritical height of R.L. 776 (that was to say, equivalent to a depth of 76 feet n the reservoir), the subsidence of the fill had given every indication of eing accelerated. That was owing to the fact that the upstream fill, lue to the submergence of its lowest parts, was in its worst possible con-lition for opposing the fluid pressure of the clay towards the reservoir. When the reservoir was full, the water pressure helped to counteract the luid pressure from the clay wall, but, at a depth of 76 feet, the balance of orces was upset in favour of the clay wall.

The alternative proposition had been to load the outer parts of the rockfill and to avoid placing any more weight on top of the clay. The value of that course lay in the retention of the clay wall to as great an extent as was then feasible, as a staunching agent against leaks through the concrete corewall. It had been anticipated that, with such deflexions in the corewall as were occurring, there were bound to be cracks near the base, and the clay might be invaluable. It had therefore to be retained if at all possible.

After full discussion and deliberation, the Board of Inquiry had decided

§ *Ibid.*

on the latter course. That accounted for the remedial measures a quoted by the Author (pp. 133 § *et seq.*) and for the somewhat grotesqu profiles of upstream fill which had to be developed and which were show in Figs. 5, Plate 1 (facing p. 208 §). The Board of Inquiry had had t deal with the situation as it found it.

The behaviour of the downstream rockfill under remedial work, a described by the Author on p. 138 §, was one of the most interestin phenomena which had come under review. It had given the Board c Inquiry considerable anxiety, more particularly as affecting that par of the corewall at and on either side of chainage 2,300. There was total length of about 400 feet of reinforced-concrete corewall betwee rigid abutments. The maximum deflexion at the time the Board o Inquiry had commenced its work was no less than 4 feet 8 inches. Th Author had recorded that it finally reached close upon 7 feet 6 inches. I was not until a widened base for the surcharge and the actual erection o the latter could be established, that the deflexion was brought practicall to a standstill. Fortunately, at that part of the concrete corewall th upstream clay wall had not slumped, due to the fact that the upstrean rockfill was on the rocky base of the old river-bed. It therefore had greater factor of stability against sliding into the reservoir, and had no yielded appreciably to the pressure from the clay wall. The staunchin value of the clay wall was therefore practically unimpaired at that part c the dam.

The behaviour of the downstream rockfill and its weakness in passiv resistance to the pressure of the concrete corewall had convinced Mr. Ritchi that loose rockfill, or any loose material which could not be consolidated b rollers, was unsuitable on the downstream side of a semi-rigid structur such as a concrete corewall.

He agreed with the Author's view (p. 132 §) that the construction of a articulated reinforced-concrete type of slab on the inner or water slope c the rockfill was a practice much to be commended, as compared with th use of a vertical concrete corewall in the centre of the dam. Mr. Ritchi suggested, however, that another alternative might have been considered namely, to employ a clay-concrete wall entirely in lieu of the cement concrete corewall. Sand, clay, and stone screenings or gravel were al readily obtainable, and a mixture in pug mills could have been made, such as was described by the Author on p. 144 §. Such a clay-concrete wall made to the dimensions customary in good practice for clay puddle walls would not be nearly so liable to slump and to exert fluid pressure as woul a pure clay wall. It would also possess a degree of flexibility not to b expected from a cement-concrete corewall. Further, it would be les liable to cracking and leakage than would the articulated cement-concrete on-slope type. There could, of course, be a cement-concrete corewal

§ *Ibid.*

below natural surface properly sealed into the rock foundations; the cement-concrete could be suitably bonded into the clay-concrete wall, which would be reared upon it. The cement-concrete sealing wall below the natural surface would also be necessary for the articulated cement-concrete-on-slope type, and would therefore be common to both. The cost of the two methods could be compared with a view to the adoption of the more economical method.

Mr. Ritchie acknowledged the skill and resourcefulness displayed by the present engineering staff of the Water Commission (including the Author) in carrying out the remedial measures specified by the Board of Inquiry. Those measures had frequently to be executed in difficult situations, aggravated by the fact that, until excavations and under-water inspections had been made, the plant and materials required could not be determined.

The total cost of £380,211 was a large sum for remedial measures, but he was quite satisfied that, in the words of the Board of Inquiry, the expenditure was "not more than warranted to enable the previous large expenditure to be fruitful of results." The abandonment of the dam, with all that it meant for irrigation and hydro-electric activities, would have been simply deplorable.

Mr. E. D. Shaw, of Camberwell, Victoria, pointed out that since the Paper was written the dam had been in successful operation for 2 years, and that further information was now available as to its behaviour. The drainage from all sources from the bank had not increased above the figure mentioned by the Author (namely, 48 gallons per minute) and varied with the height of the reservoir, the maximum flow during 1937 being 36 gallons with the reservoir at R.L. 822 feet and the minimum 17 gallons with the reservoir at R.L. 760 feet.

The deflexion of the corewall had continued, the measurements on the 1st June 1938 being 5·45 feet and 7·54 feet at chainages 1,550 and 2,300 respectively. During the last 12 months the deflexions at those two points had been as shown in Table XIV (p. 474).

The water-level in June 1938 was the lowest since 1930. The maximum deflexions occurred on the 5th April 1938, when the reservoir was at R.L. 760, as shown in Table XIV, following a fall of 27 feet during March, a rate 2·7 times that of the previous month. Evidently the pressure of the clay corewall was not able to adjust itself to the fall in the reservoir. There were steady decreases in the deflexions since that date until the 1st June, showing that the pressure of the corewall had been reduced. Those measurements indicated that the pressures of the clay corewall remained fairly constant until the 3rd May under varying water-levels in the reservoir, and decreased during that month, allowing a partial recovery of the concrete corewall to take place. The total deflexions at chainages 1,550 and 2,300, from June 1936 to June 1938, were 0·04 foot and 0·11 foot respectively. It would appear that those deflexions were due to some

TABLE XIV.

Date.	Level of reservoir: feet.	Deflexion of corewall.	
		At chainage 1,550: feet.	At chainage 2,300: feet.
2nd June, 1937	R.L. 770·7	5·44	7·51
6th July, 1937	758·8	5·43	7·52
3rd Aug., 1937	771·3	5·43	7·52
7th Sept., 1937	802·8	5·43	7·50
5th Oct., 1937	819·1	5·43	7·51
3rd Nov., 1937	822·0	5·44	7·50
7th Dec., 1937	816·5	5·43	7·51
7th Feb., 1938	797·9	5·44	7·52
1st Mar., 1938	787·7	5·44	7·52
5th Apr., 1938	760·5	5·50	7·56
3rd May, 1938	738·4	5·49	7·56
1st June, 1938	738·8	5·45	7·54

decrease in the bearing capacity of the rockfill behind the corewall, rather than to an increase in the pressure on the upstream side of the corewall.

A considerable portion of the rockfill is of Mount Pinniger mudstone of a friable nature, liable to be affected by moderate pressures and weather-action. There could be no doubt that some of that rockfill had broken down under the conditions, and that its bearing capacity had been reduced. That would be a reasonable explanation of the increasing deflexions since the dam had been completed. An inspection was made during May 1938 of the upstream toe of the dam, and it was observed that there was an extrusion of clay below the new rockfill between chainages 1,200 and 1,460 for a height of 15 feet above the normal level of the flats. That clay, being adjacent to the area of the slip in the bank in 1929, had probably come from the foundations under the original rockfill, and would indicate a probable failure of those foundations, due to a shear slide, as suggested on pp. 192 § *et seq.*

It would thus appear that the weaknesses in the original Eildon rockfill dam were (1) the inferior foundations, (2) the provision of clay in front of the concrete corewall, and (3) the inferior quality of the rockfill. An earthen dam would have been more suitable to the conditions at the site than the type adopted. With regard to the use of a reinforced-concrete layer on the upstream face of rockfill dams, as compared with the use of a concrete corewall, such a design was sound, but had many practical difficulties due to the settlement of the rockfill. It was extremely doubtful if such a design would be practicable at Eildon owing to the upstream curve in the bank, the excavation necessary at the upstream toe, and the difficult junction of such a facing with the wing-wall adjacent to the spillway.

Mr. C. P. Farie Wright, of Melbourne, observed that, at the time that

§ *Ibid.*

he had assumed control of the operations, work on the bank had been advanced to various stages between R.L. 760 and 780. However, on some points misunderstanding apparently existed.

The Author (on p. 131 §) stated that west of chainage 2,050 the fill went down to bedrock, but the cross section of dam at chainage 2,300 given in Figs. 5, Plate 1 (facing p. 208 §), showed the rockfill resting on the natural surface. The plans for the original work, dated the 28th January 1916, showed the rockfill to be founded on the natural (clay) surface as far west as chainage 2,400, where rock outcropped.

The methods of excavation, and of placing concrete, in the deepest part of the corewall foundation, as described by the late Mr. J. S. Dethridge †, consisted in driving, along the surface of the bedrock, a tunnel about 12 feet in height, placing concrete to about half that height, raising the tunnel roof, placing more concrete, and so on, until the surface was reached. It was mentioned that the inflow of underground water was much less than was usual at such depths. Similar mining methods were later adopted for the deep corewall foundations at the Tieton dam, Washington, U.S.A. ‡. The work at Eildon reservoir was begun in the drought season of 1914–15 ††. He had been informed that no bad ground had been met with in the corewall foundation, and that the formations passed through had been very firm. He knew of no tests of the bearing capacity of the seat of the bank. Under dry conditions the surface would be very hard. A test had been made under such conditions, in the early or design stages of the work, to determine the coefficient of friction by dragging cribs of Sugarloaf rockfill over the surface on which it was to be placed. It had taken a pull of 1 ton to move a weight of 1 ton; that was to say, the coefficient of friction was 1·0. It was, therefore, at that time considered that the downstream fill would have a margin of about 4 to 1 against sliding under the water-pressure. No test had been made under wet conditions. Mr. James Williamson had drawn attention (p. 195 §) to an apparent settlement in the seat of the upstream bank disclosed by a bore at chainage 1,550. Two other bores at that chainage, and one at chainage 2,365, had given similar and consistent indications.

The two quarries on Mount Pinniger had been opened in the early part of 1920, but, prior to the subsidence, they had been worked on a large scale for only 2 years or so. Material from Mount Sugarloaf comprised 78 per cent. of the original bank. He had calculated the voids in the original bank, as on the 30th June 1927, as having been slightly less than 30 per cent., after making an allowance for some possible settlement of the seat of the bank. The Author gave the weight of the rockfill as 90 lb.

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§ *Ibid.*

† Footnote (*), p. 454.

‡ "Dams and Control Works constructed by the Bureau of Reclamation," p. 44. U.S.A. Department of the Interior.

†† State Rivers and Water Supply Commission, Tenth Annual Report, 1914–15.

per cubic foot. Figures based on weighings made for the Board of Inquiry were 97 lb. per cubic foot for the Pinniger material, and 104 lb. per cubic foot for that from Mount Sugarloaf. He had checked those figures as they all seemed low, and they were apparently based on too high a percentage of voids.

In 1923 he had made a field determination of the specific gravity of various classes of rock in the bank, with results shown in Table XV.

TABLE XV.

Description of rock.	Specific gravity.	Weight per cubic foot: lb.
Mudstone	2·37 to 2·48	148 to 155
Sandstone	2·37	148
,, altered	2·62 to 2·69	163 to 168
Slate	2·66 to 2·74	166 to 171
,, with cleavage planes	2·88	180

On those figures he would set down the average weight per cubic foot as not less than 150 lb. for Mount Pinniger rock, and 164 lb. for Mount Sugarloaf rock. With 30 per cent. voids the weights per cubic foot of rockfill would be 105 lb. for Mount Pinniger rock and 115 lb. for Mount Sugarloaf material. The average for the original bank, with 78 per cent. Sugarloaf material, might be taken as 112 lb. per cubic foot. The rockfill was the run of the quarry, including fine as well as coarse material.

He had seen and handled the clay as it was brought up from the bores, and in shafts, and it had retained a stiff consistency. He might mention two instances of that. The 6-foot by 3-foot shaft against the corewall at chainage 2,427 (*Figs. 14*, p. 143 §) had been sunk through 69 feet of clay to R.L. 756; that was to say, to 67 feet below full-supply level. The clay was firm throughout, and no trouble was experienced in sinking, or while the shaft was open. In bore No. 1 near the corewall at chainage 1,550 (near the centre of subsidence), boring through clay had proceeded well ahead of the casing, and the bore was bottomed at R.L. 742, with the casing 20 feet above at R.L. 762. Nowhere did the investigations disclose that the rockfill had sunk into the clay, or that the latter had been forced into the interstices of the rockfill. The clay as placed stood with little noticeable settlement, except towards the western end of the bank, where some few feet in all of additional rockfill had been placed over it. The Author's conclusion (p. 128 §) that, owing to settlement of the clay, an additional 30 feet of rockfill had been placed over it by December, 1928, was incorrect, and was apparently based on a misapprehension of records. Mr. Wright had resided at the works until March 1928, and had

§ *Ibid.*

been in touch with them afterwards, and he knew that no such settlement had occurred. The Board of Inquiry in its Report had said, regarding the results of bores at chainage 1,550, "These disclose that whereas at this point the initial subsidence of the top of the rockfill was 26 feet, the top of the clay wall sank no less than 51 feet." That statement might be misleading, unless it was known that the "initial" subsidence of the rockfill was measured on the 28th April 1929, the sinking of the clay on the 3rd June following, and that during the whole of the intervening period, the placing of additional rockfill near and against the upstream face of the corewall had been in progress. The quantity so placed accounted for the excess depth noted by the Author. That could be roughly checked from *Figs. 12* (p. 137 §) and from Figs. 9, Plate 1 (facing p. 208 §). The former showed that, by the 3rd June, the original rockfill at chainage 1,550 had subsided 41 or 42 feet below its designed height of R.L. 840. In Figs. 9, Plate 1 (facing p. 208 §), the subsidence of the clay below its designed profile (shown by dotted lines) was 37 feet at point A, 45 feet at point B, and 42 feet in line with the top of its original 6-to-1 batter. To avoid further misunderstanding, he would say that the levels at A and B were not disclosed by bore No. 1 on the 3rd June, but by a shaft sunk against the corewall some weeks later. When he had inspected the subsidence on the 27th April 1929 it had been 1,000 feet in length, with a depth of 25 feet along the middle 200 feet, decreasing in an even sweep to nothing at the ends. The surface of the rockfill over the clay was unbroken, and was lowest against the corewall. Water-lines on the upper part of the waterslope were continuous and deflected towards the corewall. The general appearance was as though the upper part of the bank had gently settled back against the corewall. The lower part was, of course, under water. Next day the depth was 26 feet, and by the 5th May the length affected was from chainage 850 to beyond chainage 2,250, and longitudinal fissures had opened in the rockfill some distance away from the corewall. With regard to the possible collapse of the corewall, a critical locality during the first month or two was at and near the western curve, where the deflexion was upstream and increasing, and the top of the wall was overhanging up to 3 inches in a height of 10 feet at one point. At chainage 1,850 the top of the corewall moved 18 inches upstream between the 10th May and the 1st July. Longitudinal tension at that curve, following the subsidence, probably accounted for the most seriously cracked corewall joint and key, at chainage 1,950, and the cracked keys at chainages 2,000, 2,050 and 2,100 (*Figs. 13*, p. 142 §). He felt that if initial operations had not been directed towards securing the corewall rather than stabilizing the clay, the corewall might have been irreparably damaged.

After the investigations had been completed, he had believed the subsidence to be due to the sliding of the upstream rockfill on the natural

§ *Ibid.*

surface, owing to low frictional resistance, rather than to such high clay-pressures as had been suggested in the Report of the Board of Inquiry. He did not know what value had been adopted for the coefficient of friction in the case of the upstream bank, but, under conditions of prolonged submergence, it might well have fallen to 0·3, the figure given by Mr. W. P. Creager for dams on clay *, and the maximum coefficient allowed for rockfill dams, even if founded on rockfill or gravel, in the State of Arizona, U.S.A.†. In that case sliding would occur along the section of bank between chainages 1,500 and 1,900, where the natural surface was at about R.L. 744, with clay-pressures much below those suggested as possible. The resistance to sliding would decrease as the water-level fell from full-supply level to a minimum with the water-level at R.L. 770 or a little lower. That agreed well with the water-level, R.L. 776·5, at which the subsidence occurred, and with further subsidence when the water-level fell below that figure. At the same water-level (R.L. 776·5), with the same coefficient of friction and (equivalent fluid) clay-pressure, the higher western end of the bank would be slightly more stable. The critical water-level there would be lower. That might explain why the higher bank had not subsided, although it was founded on clay. The 50 feet or so of bank west of chainage 2,400 was founded on rock, as was the slope from the western end towards the emergency outlet. The toe of that slope was to some extent secured by the rockfill in the river-bed against the spillway face, up to about R.L. 750. The cracks in the valve-tower, and its distortion, were first noted in June 1927, but the tower stood without material strengthening until the remodelling was undertaken some 7 years later. That seemed to indicate the absence of a very high clay-pressure. A definite deflexion of the core-wall had been first noted in 1923, when the wall had been built to a height of R.L. 800. The deflexions had increased considerably after that.

From observations during the remedial work, the chief cause affecting the deflexion, under the pressure on the upstream side, appeared to be the settlement of the downstream fill as described by the Author on p. 138 §. The movements induced in the old downstream rockfill by the placing of the additional quantities probably reduced the passive resistance of the original fill, built up during compression and settlement. The action was possibly analogous to the cancellation, on the occurrence of a state of flux, of the abnormal stress associated with the flex condition, as shown in the experiments ‡ of Mr. J. P. R. N. Stroyer, M. Inst. C.E. The rapid increase in deflexions when additions to the downstream rockfill were commenced,

* G. A. Hoole and W. S. Kinne, "Reinforced Concrete and Masonry Structures," p. 538. New York, 1924.

† "Code Governing the Design and Construction of Dams in Arizona." Approved 10 Oct. 1932. State Engineer's Office.

§ *Ibid.*

‡ "Earth-Pressure on Flexible Walls." Journal Inst. C.E., vol. 1 (1935–36), p. 94 (November 1935).

and the steadying-up after 1930, when they were practically completed, pointed to some such cause. An interesting example of a thin reinforced-concrete corewall was that of the Tieton dam*. That corewall at its maximum section was 100 feet in depth below the surface, up to which it was 5 feet thick, and rose 220 feet above it, being 1 foot thick at the top. Deflexions had been checked and remedied by loading, at each lift of 5 feet 8 inches. The horizontal pressure of the clay core was measured by pressure-gauges installed in inspection-shafts. Some fracture had, he believed, occurred at or near the base of the wall.

Following the subsidence at Eildon, Mr. Wright had resided on the works until the arrival of the Author in August 1929, to take up the duties of Resident Engineer, and Mr. Wright continued to do so for a great part of the following 15 or 18 months, by which time the remodelling of the bank was well advanced. When he retired in June 1933, the main works still to be carried out were the installation of gates on the spillway, and the remodelling of the valve-tower.

In conclusion, regarding the period prior to August 1929, he would like to acknowledge the valuable assistance received from Mr. W. A. Robertson, M.C.E., M. Inst. C.E., and the work of Mr. H. H. C. Williams, B.M.E., Assoc. M. Inst. C.E., as Assistant Engineer.

The Author, in reply to the Discussion and Correspondence, observed that the rockfill dam had far outstripped its pioneering predecessor, and had entered the sphere of scientific design. The Eildon dam was one of the earliest of its type in Australia, and the Author was greatly appreciative of the interest that had been evinced by the description of its subsidence described in his Paper. The Discussion and Correspondence had brought out many points in design and aspects of construction, which, when studied in the light of present-day practice, would be another step towards the stabilization of ideas with respect to rockfill dams.

Opinion regarding the cause of the subsidence supported to some extent the theory of the horizontal slipping on the greasy surface due to the outward pressure of the clay, but was more inclined to support the theory of the shear-slide failure of the foundation along a cylindrical plane, according to the theory of Dr. K. von Terzaghi, and Messrs. W. Fellenius, H. Krey, and others. There was evidence in support of both theories.

In the initial stages of the movement, the rockfill was comparatively undisturbed on the surface, and had the appearance of having "settled back against the corewall" (see Mr. Wright's description, p. 477), as would be expected from a rocking on the base, while in the later stages, the longitudinal and outward movement of the rockfill suggested an outward slipping on the greasy seat of the bank.

An inspection of the natural surface immediately upstream of the toe

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* Footnote (‡), p. 475, *and* "Puddle Core Investigations at Tieton Dam." *Engineering News-Record*, vol. 97 (1926), p. 544.

of the bank during the dry-reservoir period of 1930 had disclosed upheaval of the foundations, nor was there any indication of such a movement, as mentioned by Mr. Lewis, in 1927. It was noted, however, that the cylindrical plane of shear slide would be covered by the rockfill as shown in Mr. Cooling's diagram (*Fig. 28*, p. 199 §). The extrusion of clay observed between chainages 1,200 and 1,460 by Mr. Shaw during a recent dry-reservoir period could be applied to support both theories, for, according to the one, the clay would have been squeezed into the rockfill ahead of the slip, and, in the light of the other, it would have been tipped up above the natural surface, and ultimately exuded by the weight of the superimposed fill.

The experience of Mr. Lewis at the Bon Accord dam in South Africa bore out to a marked degree Mr. Cooling's reasoning, as illustrated in *Fig. 28* (p. 199 §), and increased the evidence in favour of the cylindrical shear-slide as the real cause of the Eildon failure. The possibility of incipient movement of that nature having occurred in 1927 was supported to some degree by the necessity for the addition of small quantities of stone to the upstream side of the corewall to make good settlement; that tipping had been continued off and on for some months prior to the subsidence.

The method of levelling between plates set in the dam, described by Mr. W. J. E. Binnie as having been used at the Shing Mun dam, was interesting in that the foundation- and bank-settlement were thus separately determined.

The superior quality of the Sugarloaf stone, which largely predominated in the older portion of the bank would, after so many years, be approaching closely to its full settlement. The expected disintegration of the Pinniger stone would be mostly on the surface. Within the embankment, however, where the less durable stone would be protected from weathering to a large extent, the effect would be much less marked. That applied particularly to the downstream side where the corewall depended so much upon a solid bank. On the water-face, the repeated wetting would possibly accentuate that action, but the absence to date of such a disintegrating effect on the upstream side had been noticeable.

The weight of the rockfill in the dam was generally accepted as about 90 lb. per cubic foot. Small quantities had been weighed during the inquiry, the average specific gravity of the stone having previously been determined. The results of the experiments were:

Rockfill.	Volume weighed: cubic feet.	Total weight: lb.	Unit weight: lb. per cubic foot.	Average specific gravity.	Solid weight: lb. per cubic feet.	Voids in specimen weighed: per cent.
Pinniger .	15·76	1,530	97·08	2·49	156	38
Sugarloaf .	15·86	1,650	104·04	2·64	165	37

§ *Ibid.*

The rock had been placed in the container by hand so that the void ›ercentage could be assumed to be lower than in loosely-tipped rock.

The average percentage of voids in the hand-packed rock on the latest 'rench rockfill dams in Algeria,[1] containing boulders of from 2 tons to 1 tons weight, with occasional 16-ton boulders, and smaller rocks of from 0 lb. to 750 lb. weight in the voids, varied from 25 per cent. to 32 per ent., so that 30 per cent. was considered too low for loosely tipped "one-nan" stone. With 78 per cent. Sugarloaf stone the weight given would epresent 44 per cent. voids, which was in keeping with the figure suggested y Mr. Ritchie (p. 471), in which case 90 lb. per cubic foot would be fairly lose to the true weight of the bank.

Mr. G. M. Binnie had observed that the consolidation of rockfill might e accomplished by the sluicing of clean rocky fines into the voids when the ocks of the mass had become firmly seated on their bearing surfaces. The chief object was to insure that the fine material did not cushion the arger stones during settlement, as had been the case at San Gabriel No. 2 lam, where subsequent wetting had washed out the fine supporting naterial. Those points were discussed at length in Mr. J. D. Galloway's Paper.[2]

The necessity for consolidating the fill had been well established in the nore recent examples of that type of dam, where successful results had een obtained by eliminating settlement. The rolling of the fill in the Lock Treig dam, referred to by Mr. Halcrow, had introduced a method vhich could be used to advantage with stones of small maximum weight. The relative weight of the roller and that of the largest stone would be mportant; with a 10-ton roller and a stone of 2 feet maximum dimension he stone could be subject to a point load of the order of, say, 50 times its veight, and under such compression a good-quality stone would be so irmly seated as to preclude further settlement without fracture and eseating of the rock. The cross sections in *Figs. 33* (p. 482) showed the ed-rock and general foundation conditions from chainage 2,200 west-vards; they had been traced from the actual progress cross sections prepared during construction. The records showed that between November 1929 and May 1930, the quantity of rockfill tipped on the downstream side had been 154,700 cubic yards.

In verifying the statement in the Board's report referred to on p. 477, the Author had checked the sections and had made a computation of the quantity of fill between the level of the original rock at chainage 1,550 on the 3rd June 1929, and that of the new fill adjacent to the core wall on that date, which disclosed that, during the initial stages of the "principal slip" (as described by Mr. Williamson on p. 195 §), approximately 18,000

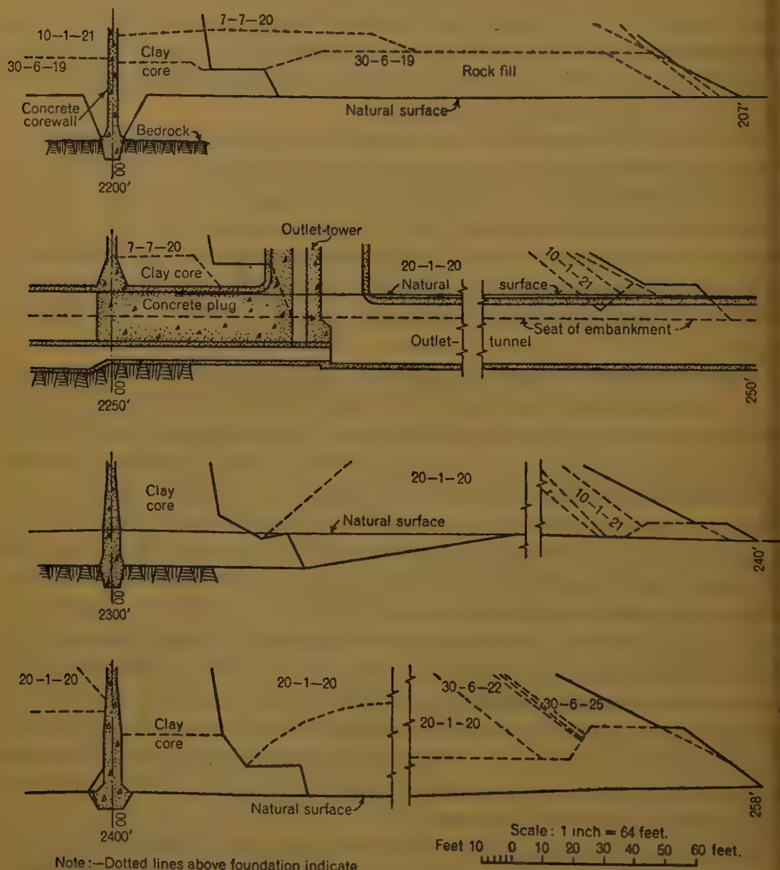
[1] I. Gutmann, "Algerian Dams of Placed Rock Fill." *Engineering News-Record* vol. 119 (1937), p. 889.

[2] Footnote (*), p. 460.

§ *Ibid.*

cubic yards had been tipped, which agreed fairly closely with the records which showed that 19,700 cubic yards had been placed by the 3rd June. The rock tipped upstream of the corewall had a portion of 9,320 cubic yards taken from the Pinniger quarries during 1928, of which it was recorded that 3,900 cubic yards had been placed during the Spring of that year on

Figs. 33.



Note:—Dotted lines above foundation indicate progress to the dates shown.

the downstream side. As Mr. Galloway pointed out, loading of the clay would make no material difference to the ultimate result, as the clay core itself would be heavier than the rockfill and would contribute more to the subsidence, which probably had its beginnings in the slight movements of some months previously, which might have been the reason for the deficiencies occurring in the fill at the corewall.

That the Eildon embankment should have been consolidated was now

a generally-accepted fact. The wetness of the downstream foundations had played no part in the corewall deflexion, but it was considered a possible source of danger, as pointed out by Professor Sir Robert Chapman, and while the lack of consolidation had been largely a contributing factor in the deflexion of the corewall, in that there had not been sufficient "passive resistance" to oppose movement, the existence of an unbalanced pressure from the upstream side appeared to have been well demonstrated.

By placing the surcharge against the corewall it had been expected that consolidation of the fill would have taken place due to its weight, and that an active pressure against the wall would have been developed; that was thought preferable to adding the fill to the slopes as Mr. Williamson suggests on p. 195 §; the object had, however, been defeated to some extent by the downstream lean of the corewall, which was accentuated by settlement as shown in *Fig. 12* (p. 137 §).

The removal of the central part of the bank as suggested by Mr. Sandeman (p. 195 §), had not been considered; it had been necessary to restore the full capacity of the reservoir as soon as possible to insure the supply of water to meet irrigation commitments.

The clay in the puddle core was similar to that in the foundations of the dam, the borrow-pits for the former being adjacent to the dam. The lowering of the shear strength due to the continued soaking after the first filling in 1927 would, as Sir Henry Japp and Mr. James Mitchell pointed out, render it more susceptible to the cylindrical shear failure under the differential loading of the fill at the core and toe. Mr. East had drawn an interesting comparison between the Eildon design and that of Melton dam, on which the former was supposed to be based. The absence of clay overlying the bed-rock at the Melton dam, together with the consolidation of the fill, had insured its success, but had obscured the dangerous possibilities of a clay-shear failure when the conditions were not exactly reproduced. It was pointed out that the original profile of the Eildon design had been shown as being founded on rock.*

Whilst the old tests for clay used in puddle cores had served very well up to date, and might still be taken as a guide in their selection, modern application of soil-mechanics should enable clays to be classified, and their behaviour fairly accurately predicted.

The Eildon clay had been tested by Messrs. A. H. Gawith and H. T. Loxton of the testing laboratory of the Country Roads Board of Victoria. The samples were taken from the borrow-pits on the upstream and downstream sides of the dam, from which the puddle clay had been taken. The Adelaide tests dealt with clay with no less than 30-per-cent. moisture, and the present series had been extended to include clay with 23-per-cent.

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§ *Ibid.*
* J. S. Dethridge, "Irrigation Works and Practice in Victoria." Trans. Inst. E. Aust., vol. 2 (1921), p. 93.

moisture-content. The results were set out in the form of an Appendix (pp. 489 *et seq.*).

In using Mr. Bell's method for determining lateral pressure, it should be noted that from the tabulated values of "k" and "a" the value of H at certain depths had been computed according to the formula stated and from those had been deduced the weight per cubic foot of a "perfect fluid" which would produce equivalent pressures. Mr. Bell observed that the slopes in the diagram were flatter than in his original diagrams; the results were the same, however, as the scales were adjusted to suit the slopes.

The importance of the collaboration between the soil-physicist and the engineer, as in India, was being recognized to a greater extent in Australia than hitherto, and the recently published Annual Report of the Central Board of Irrigation referred to the treatment of clays in irrigation-works under conditions which might easily apply to Victorian practice.

The corewall at Melton, after water had been stored for the first time had been displaced downstream 13 inches in 600 feet; prior to that there had been no deflexion. It had increased to 1 foot 8 inches in 13 years and had attained a maximum of 1 foot 9 inches, as disclosed by recent measurements (p. 458).

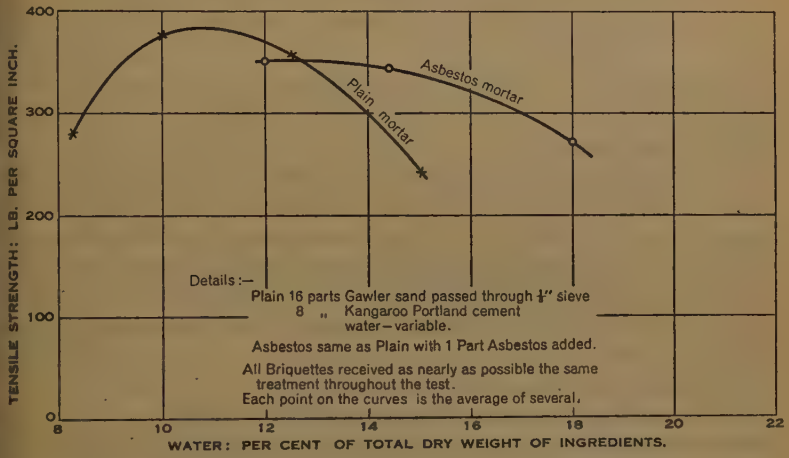
Both Mr. Kelso and Mr. Lewis had drawn attention to the possibility of corrosion of the Eildon corewall reinforcement. The crack at the junction of the core and approach walls at chainage 2,427 had been under constant observation, and no appreciable increase had taken place there. The increase in downstream deflexions had been slight, as seen from Table XIV (p. 474) and Mr. Shaw's comments. It was expected that further cracking due to those downstream movements would be small and would take place throughout the length of the wall; each individual crack might then be fine enough to seal itself and to prevent corrosion.

The corewall appeared still to be able to accommodate itself to adjustments in the rockfill without damage to the reinforcement, and such adjustments should now be small. Some tensile and shrinkage tests had been carried out on the asbestos-cement mixture, the former at Adelaide University in 1929—of which the results had been plotted in the form of a graph (*Fig. 34*)—and the latter recently in the Commission's Testing Laboratory, the results of which were as follows:—

Cement—Sand—Asbestos Fibre Mixture. Proportions by weight: cement = 8; sand = 16; asbestos = 1. The mixture was gauged with water to a plastic condition and was moulded into the form of bars 10 centimetres in length. Two series of experiments were made, the average of three bars being given as the figure for the series: series 1 comprised asbestos fibre not previously moistened, and series 2 comprised asbestos fibre previously soaked in water. The experiments were conducted at a temperature 14·5° C. ± 2·5°; and corrections were made in the readings

for temperature-variation. The bars were exposed to the natural humidity of the air in the laboratory, and no humidity-control was imposed. It was not feasible to conduct measurements until specimens were hard

Fig. 34.



TENSILE TEST ON ASBESTOS-CEMENT MORTAR AS USED FOR SEALING CRACKS IN COREWALL.

enough to bear some pressure, and hence the measurements were started at the age of 1 day. It was found that no contraction had taken place prior to the first day. The results were as follows :—

Age :	1 Day.	7 Days.	14 Days.	20 Days.	27 Days.	32 Days.
Series 1. Length of bar : centimetres . . .	10·0	10·00025	—	10·00037	—	10·00021
Series 2. Length of bar : centimetres . . .	10·0	—	9·99968	—	10·00043	—

In answer to Mr. H. G. Lloyd's enquiry regarding the cement used at Eildon, he gave the following information :

Weight of jute bag of cement, 125 lb. (eighteen bags to the ton).
„ paper „ „ 94 lb.

The paper bags were usually taken as containing 1 cubic foot of cement. The jute bags were used for their salvage value and general utility in construction ; they cost 3*s*. per ton more than the price of paper. (The Commission appeared to be the only users of the jute bag in Victoria.)

Price per ton, 1929–30, 92*s.* free on rail at Melbourne.

" " 1934–35, 86*s.* 4*d.* " " "

" " from end of financial year 1930–31 to end of financial year 1933–34, 88*s.* free on rail at Melbourne.

Freight-charge, Melbourne to Alexandra, 23*s.* per ton.

Cartage-charge, Alexandra railway-station to cement-shed (18 miles), 17*s.* per ton.

The contract price was with the makers near Geelong (Fyansford), and was at present 80*s.* 10*d.* per ton free on rail on Melbourne, or 74*s.* 6*d.* per ton free on rail on Fyansford.

In the early design a vent-shaft was provided to aerate the outlet-pipe, but as it joined the pipe about 10 feet downstream it was inefficient in preventing the formation of vacuum below the gates. The increased pressure due to that vacuum, and that beneath the gate, were contributing factors in the difficulty experienced in raising the valve-leaves, but the damage to the guides caused by the vibration of the gates was thought to be the main reason; the improved design of a gate-valve as described by Mr. Williamson had much to commend it.

Mr. David Lloyd's observations regarding the variations in loss over catchment-areas, when applied to the Eildon catchment, had given figures in agreement with the figures that it had been possible to deduce from records, which on account of the remoteness and inaccessibility of the catchment were necessarily meagre. The mean temperature had been recorded at Alexandra (altitude 720 feet) 18 miles west of (below) the dam and was 57° F. No temperature-records were available for the catchment-area as a whole, the elevation of which varied from 900 feet to 6,000 feet at the Divide; it had two prominent features within its boundaries, Mount Torbreck (4,800 feet) and Mount Buller (about 6,000 feet), snow-covered from about July to November.

The divers employed on the outlet-works comprised both naval men and divers whose experience had been gained on bridge and harbour works. The diving was carried out strictly in accordance with the Decompression Tables and other safety clauses in the Naval Regulations. The maximum depth in which they worked was from 110 to 115 feet of water, the maximum reservoir-level during 1933, 1934, 1935, and 1936 having been R.L. 817·5, 825, 823, and 823·19 respectively. The decompression schedules for a number of divers on shift-work were rather complicated at times. There was, at first, a tendency in one or two of the younger divers to under-estimate the importance of decompression, but that was soon overcome. The cross beams at appropriate levels provided the necessary exercising platforms. No case of "bends" occurred during the operations.

Mr. Nimmo had given figures relating to energy-dissipation within the outlet-pipes; that dissipation had been demonstrated to a marked degree

during an emergency discharge in May 1933, when the excessive vibration at the breeches pipes which occurred at part opening increased with increased discharge to such an extent that it was considered undesirable to continue. It was not until the completion of the installation that the valves were tested to full opening; the vent-pipes had then been placed, and the severe detonations previously heard at the breeches pieces were reduced to a deep vibration which might arise from the diaphragm at the bifurcation.

Gaugings of the Goulburn river had been taken for over 50 years prior to 1929, and the largest floods recorded had been gauged at the Eildon reservoir in September 1916 and June 1917. They were of such intensity and duration as to be considered of exceptionally rare occurrence. Both those floods had followed storms during which from 3 inches to 3½ inches of rain had fallen in 24 hours at one or more gauging station on a previously wetted catchment. An average rainfall of 3 inches was assumed to have fallen over the whole catchment during the previous 24 hours.

A comparison of the Burrinjuck and the Goulburn floods is made in Table XVI.

TABLE XVI.—COMPARISON OF THE BURRINJUCK AND GOULBURN FLOODS.

Flood.	Maximum intensity (average for 2 hours):		Maximum discharge in 24 hours: acre-feet.	Total dischage in 4½ days: acre-feet.
	Cusecs.	Cusecs per square mile.		
Goulburn 1916 . .	80,000	53	125,000	242,000 (4 days)
Goulburn 1917 . .	70,000	—	—	305,000
Proportionate Burrinjuck	146,000	97	196,000	275,000 3 days only (duration of high flood)
Combined 1916 and 1917	150,000	100	248,000	547,000

The combined 1916–1917 flood was adopted as a flood of maximum intensity to be anticipated from the catchment, which, with a discharge of 4,000 cusecs through the outlet-valves, would give a freeboard of 4 feet 8 inches.

No model-experiments had been conducted in connexion with the discharge-capacity of the spillway. The manufacturers of the steel gates had constructed a 6-foot model which resulted in the placing of the skin-plate on the downstream side to avoid flotation, which in the model, with the plate on the upstream side, prevented the closing of the gates. In that case the gates had been designed by the makers originally for the Hume dam on the river Murray. Beyond the possibility of a slight variation in the shape of the crest (which would not have materially increased

the discharging capacity), model-experiments would have been of little use in the investigation of the action of the gates.

The contributions to the Correspondence by Professor Sir Robert Chapman, C.M.G., M.A., B.C.E., and by Mr. H. H. Dare, M.E., and Mr. E. G. Ritchie, MM. Inst. C.E., were particularly valuable as coming from the members of the Inquiry Board who were appointed by the Government to advise on the remedial measures; whilst those of Mr. C. P. Farie Wright, M.C.E., and Mr. E. D. Shaw, M.C.E., MM. Inst. C.E., who were responsible to the Commission (the former until June 1933, and the latter from then until the completion of the operations) for the carrying out of the works, were also very informative.

The Author was indebted to Mr. L. R. East, M.C.E., M. Inst. C.E., Chairman of the Commission, for permission to consult Head Office records, and to Mr. W. A. Robertson, M.C.E., M. Inst. C.E., Commissioner, for making available for reference the results of his investigations. Thanks were also due to Mr. L. F. Loder, M.C.E., Chief Engineer of the Country Roads Board for the use of the Board's laboratory, where Mr. A. H. Gawith, B.C.E., and Mr. H. T. Loxton, B.C.E., B.Sc., carried out the tests on the Eildon clay.

APPENDIX.

Soil-Tests on Clay Samples from Eildon Reservoir.

Samples.

Two samples of clay were tested: (*a*) from borrow-pits on reservoir bank upstream; (*b*) from borrow-pits on river flat below reservoir downstream.

Tests Carried Out.

The tests carried out were: (1) simplified soil-tests; (2) mechanical analysis; (3) consolidation (Proctor tests); (4) compression and (5) permeability (Terzaghi tests); (6) shear and cohesion tests, using a Hveem stabilometer.

Discussion of Results.

(1) *Simplified soil-tests.*—The usual series of soil-tests was carried out on the samples (Table XVII).

Table XVII.—Results of Tests on Two Samples of Clay from Eildon Dam, forwarded by the State Rivers and Water Supply Commission.

Marked.	Upstream.	Downstream.
Lower liquid limit	52·6	68·0
Lower plastic limit	21·5	22·6
Plasticity-index	31·1	45·4
Field-moisture equivalent: per cent.	29·9	28·1
Shrinkage-limit: per cent.	16·4	13·7
Shrinkage-ratio	1·81	1·89
Volumetric change (between field-moisture equivalent and shrinkage-limit)	24·4	27·2
Linear shrinkage (between field-moisture equivalent and shrinkage-limit)	7·0	7·7
Specific gravity (calculated)	2·57	2·57
Flow-index	15·0	22·9
Toughness-ratio	2·08	1·98

(2) *Mechanical analysis.*—Two subdivisions of a soil into sand, silt, and clay were in common use, namely:

	Bureau of Public roads.		International.	
Coarse sand	2·0 to 0·25	millimetres.	2·0 to 0·2	millimetres.
Fine sand	0·25 to 0·05	,,	0·2 to 0·002	,,
Silt	0·05 to 0·005	,,	0·02 to 0·002	,,
Clay	less than 0·005	,,	less than 0·002	,,

The figures given were those of the Bureau of Public Roads.

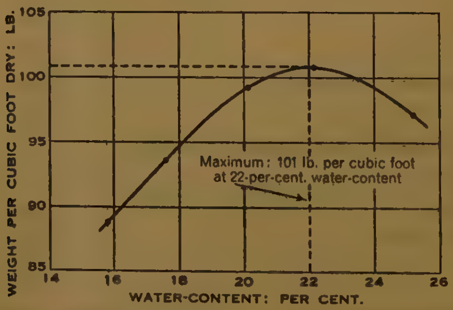
The Mechanical analysis was by the Boucouyos hydrometer method (B.P.R. classification).

	Upstream.	Downstream.
Passing No. 8 B.S. mesh : per cent.	100	100
Coarse sand (2·0 to 0·25 mm.) : per cent.	1	0
Fine sand (0·25 to 0·05 mm.) : per cent.	9	6
Silt (0·05 to 0·005 mm.) : per cent.	34	29
Clay less than 0·005 mm. : per cent.	56	65
Colloids less than 0·001 mm. : per cent.	42	50

(3) *Proctor consolidation-tests.*

Sample.	Moisture : per cent.	Weight per cubic foot (dry) : lb.
From reservoir bank (upstream) . . .	15·7	89·1
	17·7	93·6
	20·2	99·3
	22·2	100·8
	25·2	97·0
Maximum values from *Fig. 35* . . .	22	101

Fig. 35.

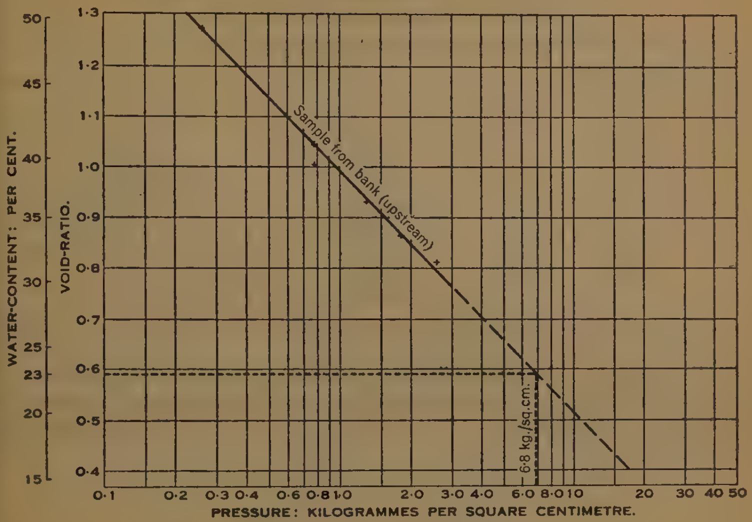


The Proctor consolidation-test consisted of ramming the soil into a mould in a standard manner. The results gave the moisture-content at which maximum consolidation was obtained (results parallelled the compaction of a bank using sheeps-foot rollers). The maximum value of 101 lb. per cubic foot dry, or 124½ lb. per cubic foot wet for a moisture-content of 22 per cent., agreed with the density of 123 lb. per cubic foot found in the bank.

(4) *Compression-test* (*Terzaghi*).—The results were plotted in *Fig. 36*, the logarithm of the load against the void-ratio giving a straight line. The maximum load permissible on the apparatus was 2·6 kilogrammes per square centimetre; it was really too low, but by extrapolating the curve a load of 6·8 kilogrammes per square centimetre was obtained for a moisture-content of 23 per cent. (that was, with a void-ratio = 0·61), equivalent to 110 feet of bank.

(5) *Permeability-test* (*Terzaghi*).—The test was carried out in the Terzaghi apparatus

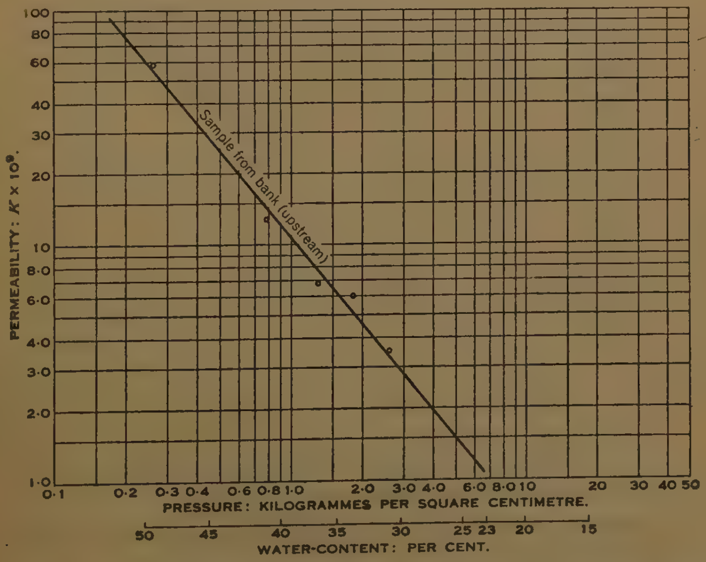
Fig. 36.



COMPRESSION-TEST (TERZAGHI).

The results were given in Table XVIII (p. 492). They were plotted in *Fig. 37*, the logarithm of the permeability against the logarithm of the load giving a straight line.

Fig. 37.



PERMEABILITY-TEST (TERZAGHI).

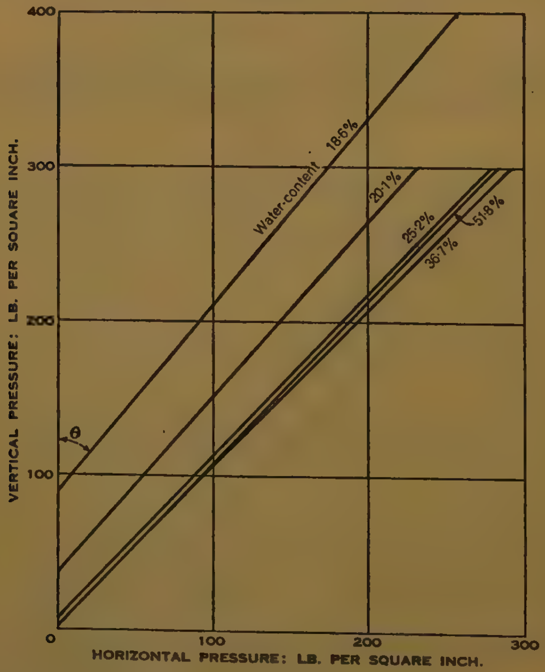
On the same graph could be plotted scales of void-ratio and water-content. Again the curves could be extrapolated to give values for the lower void-ratios.

TABLE XVIII.—COMPRESSION TEST (TERZAGHI).
(Sample from reservoir bank upstream.)

Load : kilogrammes per square centimetre.	Void-ratio, $\frac{d - d_0}{d_0}$.	k : centimetres per second.
0	1·327	—
0·26	1·273	$58{\cdot}1 \times 10^{-9}$
0·78	1·049	$12{\cdot}8 \times 10^{-9}$
1·30	0·932	$6{\cdot}8 \times 10^{-9}$
1·82	0·866	$6{\cdot}0 \times 10^{-9}$
2·60	0·812	$3{\cdot}5 \times 10^{-9}$
1·30	0·812	—
0	0·875	—

(6) *Shear-tests.*—A Hveem stabilometer was used to measure the principal stresses in a pat of the clay. A cylindrical sample of the clay was retained in a rubber diaphragm-tube. The pressure exerted on that diaphragm while the sample was com-

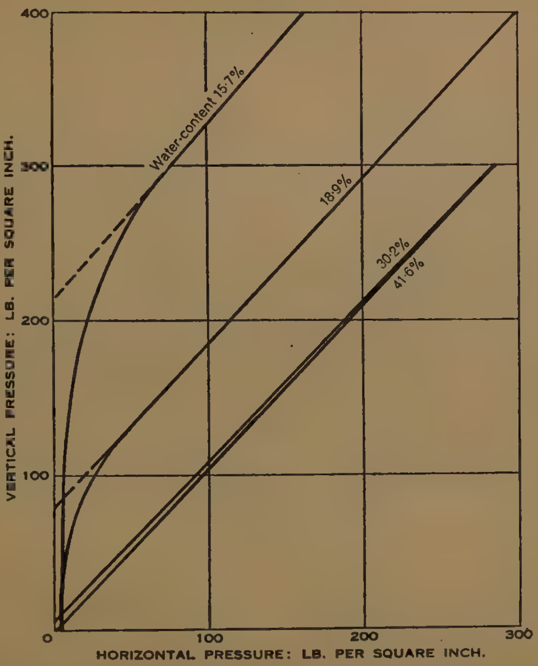
Fig. 38.



STABILITY-TEST. (SAMPLE OF CLAY FROM RESERVOIR BANK UPSTREAM.)

pressed by two solid pistons was transmitted by a liquid which filled the annular space surrounding the diaphragm to a pressure-gauge. The test was carried out at a number of moisture-contents. The experimental results were plotted in *Figs. 38* and *39*.

Fig. 39.



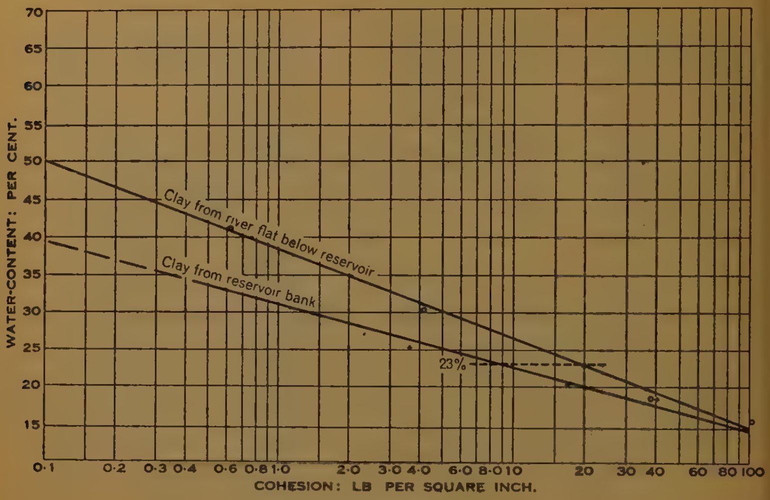
STABILITY-TEST. (SAMPLE OF CLAY FROM RIVER FLAT DOWNSTREAM.)

The following Table compared the results obtained by that method with those obtained by Mr. A. L. Bell using the normal shear-apparatus.

	Mr. A. L. Bell's experiments.				Country Roads Board's experiments.			
	K	ϕ	P (50-foot bank).	P (100-foot-bank).	K	ϕ	P (50-foot bank, 42·6 lb. per square inch).	P (100-foot bank, 85·2 lb. per square inch).
30 per cent. moisture . . .	820	5° 10′	3,500	8,500	720	1° 30′	5,030	10,800
35 per cent. moisture . . .	396	2° 38′	4,700	10,160	288	—	—	11,400
40 per cent. moisture . . .	333	1° 05′	5,100	10,870	10	1° 20′	5,800	—

If a critical plane in the specimen were assumed at an angle of $(45-\phi/2)$ degrees, then from the slope of the above graphs ϕ might be calculated, and from the intercept on the vertical pressure axis the cohesion K might be obtained (Table XIX, p. 494). If the logarithm of the cohesion were plotted against the water-content, approximately a straight line was obtained. (*Fig. 40*, p. 494.)

Fig. 40.



STABILOMETER-TEST.

TABLE XIX.—COMPUTATION OF SHEAR AND COHESION.

(*a*) *Clay from reservoir bank upstream.*

Water-content: per cent.	51·8	36·7	25·2	20·1	18·6
Tan θ (slope of graph with vertical pressure axis)	0·942	0·981	0·946	0·861	0·824
Intercept on vertical axis.	1·48	4·3	7·5	36·1	89·9
Tan (45° − ϕ/2)	0·971	0·991	0·973	0·927	0·908
ϕ.	1° 42′	0° 32′	1° 34′	4° 20′	5° 32′
Cohesion: lb. per square inch	0·716	2·1	3·65	17·00	40·8

(*b*) *Clay from river flat downstream.*

Water-content: per cent.	41·6	30·2	18·9	15·7
Tan θ	0·954	0·973	0·937	0·889
Intercept	1·2	8·5	79·7	218
Tan (45° − ϕ/2)	0·976	0·986	0·968	0·942
ϕ	1° 24′	0° 48′	1° 52′	3° 23′
Cohesion: lb. per square inch	0·61	4·2	38·5	102·8

The results of the series of tests on both samples were shown in Table XX.

Those two samples of clay had equal cohesion at 15-per-cent. moisture-content, but as indicated by the lower value of its lower liquid limit and its lower flow-index, the cohesion of the sample of clay from the reservoir bank downstream decreased more rapidly with increase in water-content than the sample from the river flat (*Fig. 40*).

TABLE XX.

	Clay from borrow-pit on reservoir bank upstream.			Clay from river flat downstream.		
Water-content: per cent.	20	23	30	20	23	30
Void-ratio	0·78	0·89	1·68	0·78	0·89	1·68
Vertical pressure (Terzaghi test): kilogrammes per square centimetre	10·0	6·8	2·9	6·8	5·0	2·7
Height of bank equivalent to this pressure: feet	164	110	48	110	82	44
Permeability $\times 10^{-9}$	0·6	1·0	2·9	0·8	1·1	2·0
ϕ	4° 30′	2° 30′	1° 30′	2° 30′	1° 30′	1° 00′
Cohesion: lb. per square inch	22	8	1·2	33	20	4·8
Stability measurement:—						
Horizontal pressures in lb. per square inch:—						
For vertical pressure = 42·6 lb. per square inch (50 foot bank)	5	24	35	—	13	35
For vertical pressure = 85·2 lb. per square inch (100-foot bank)	40	70	78	24	50	74

Paper No. 5110.

"An Experimental Investigation of the Effect of Bridge-Piers and other Obstructions on the Tidal Levels in an Estuary." †

By Professor ARNOLD HARTLEY GIBSON, D.Sc., LL.D., M. Inst. C.E.

Correspondence.

Mr. Herbert Addison, of Giza, Egypt, had found it interesting to construct diagrams showing the longitudinal profile of the water-surface in the models. One of them was based on the tide-curves reproduced in *Figs. 6* (pp. 220 § and 221 §); since in all the curves the same datum for time and the same datum for levels were used, it was easy to plot the surface-profiles at various periods before and after high water. Only points relating to the unobstructed estuary were chosen: at Avonmouth,

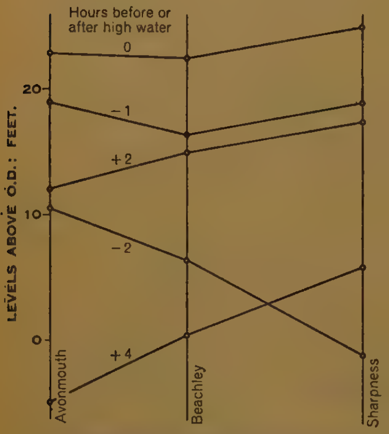
† Journal Inst. C.E., vol. 8 (1937–38), p. 210 (March 1938).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

above Beachley, and at Sharpness. The profiles appeared (*Fig. 9*) to be normal in form, and they showed graphically how the inertia forces accelerating or retarding the water affected the levels at the head of the estuary.

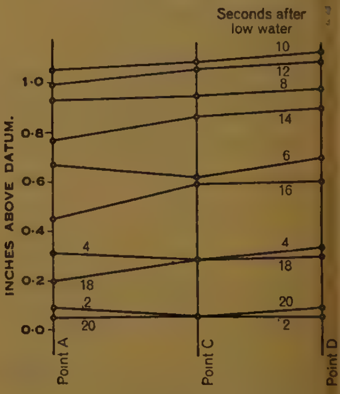
The profiles relating to the symmetrical-estuary model, based on *Figs. 7* and *8* (pp. 223 § and 224 §), and concerning points A, C, and D (open estuary) were more difficult to understand (*Fig. 10*). The remarkable thing about them was that in every profile except the one taken at 2 seconds after low water, the water-level at the head of the estuary (point D) was higher than the level at the mouth (point A). That was contrary to what would be imagined would happen in ideal conditions

Fig. 9.



Horizontal scale: approx. 1 inch = 8 miles.

Fig. 10.



Horizontal scale: 1 inch = 32 inches.
Inches 10 5 0 10 20 inches

where the level at the head would be above the level at the mouth of the estuary at the end of the flood and at the beginning of the ebb, whilst at the end of the ebb and at the beginning of the flood the level at the head of the estuary would be below the level at the mouth. It would be very helpful to have the Author's comments on that point.

Could the Author give any figures for the afflux produced by the various kinds of obstruction? What, for example, was the maximum estimated afflux, for the full scale, produced by the piers of the proposed bridge over the river Severn which gave an 8 per cent. obstruction of the channel?

Mr. R. F. Hindmarsh observed that a record of actual tidal observations taken on the river Tyne might be a useful addition to the results given in the Paper and the conclusions reached.

At the present time there were in all eight bridges between the north

§ *Ibid.*

and south banks of the river at or above Newcastle, which was 9 miles from the sea, the bridge farthest from the sea being Newburn bridge at a distance of 18 miles. All the bridges except the latest—the Tyne bridge—had piers in the river, the greatest obstruction being at the Swing bridge at Newcastle, where it amounted to about 25 per cent. The Tyne Improvement Commission, whilst safeguarding the interests of navigation, had never offered objection to the erection of the bridge-piers at present in the river.

Automatic tide-gauges were in use at the river entrance and at Newcastle, and actual tide-observations were taken daily at Newburn bridge and at Ryton, about 1 mile above Newburn bridge. The following was a record obtained by averaging a number of observations taken in August 1930 and in December 1937.

	North and south piers, Tyne entrance.				Swing bridge, Newcastle.				Ryton and Newburn bridge.			
	Time: hours.	mins.	Height: feet.	inches.	Time: hours.	mins.	Height: feet.	inches.	Time: hours.	mins.	Height: feet.	inches.
					Tide-observations, August 1930.							
H.W.O.S.T. .	8	12	13	2½	8	15	13	10	8	16	14	1
L.W.O.S.T. .	8	22	2	11½	8	36	3	2	8	45	3	2
					Tide-Observations, December 1937.							
H.W.O.S.T. .	4	8	13	7	4	0	14	5	4	25	14	11½
L.W.O.S.T. .	10	5	2	6	10	2	3	0	10	31	2	6

In the vicinity of several of the bridges the level of the bed of the river was below the level of the bed above and below the bridge. There was no change in the river between the years 1930 and 1937 so far as obstructions or clearance of obstructions to tidal flow was concerned.

Mr. G. A. Maunsell observed that the Author's experiments went to prove that the amplitude of the tidal wave at the head of an estuary was not necessarily lowered by placing a partial obstruction across the estuary lower down, a conclusion which was to some extent contrary to what might have been anticipated according to pre-conceived ideas.

A few years ago he had studied movements which had occurred in the estuary of the Sittang river over a period of the preceding 30 years, and some of the movements in question appeared to have distinct bearing upon the subject of the Paper, and if not to corroborate, at any rate to parallel the Author's conclusions.

The Sittang estuary, situated at the head of the Gulf of Martaban in Burma, was probably the place where tidal and estuarian phenomena occurred on the grandest and most bizarre scale of anywhere in the world.

It might, indeed, be regarded as a natural laboratory where the most astounding changes were continually in progress.

At the head of the estuary, where the waterway was only about ⅓ mile wide, was situated the Burma Railways bridge, having eleven spans of 150 feet. On the east bank 10 miles below the bridge there was a village called Kyaikkatha, and to an observer there the channel of the Sittang in 1930 had the appearance of an estuary 3 miles wide, bordered at low water by great expanses of mudbank with extensive shoals of sand exposed in the waterway. The water between the sand-shoals was spread out in wide shallow sheets, through which, during the period of low water (which lasted for 6 or 7 hours), the ebb flow went on continuously but without much apparent lowering of water-level.

Over those expanses of mud, sand, and water the incoming tide advanced suddenly. There was no preliminary backing up of the water-level before the turn, and no period of slack water. The crest of the advancing tidal wave appeared running over the top of the ebbing water, over-riding the banks of mud and sand and accompanied by noise and commotion, and bringing in its wake an immediate reversal of flow. Following the reversal the current flowed upstream at high speed, the whole channel became filled with swirling turbid water and the water-level rose, and continued to rise rapidly until in the course of 1 or 2 hours the whole estuary was running brimful between its banks. There followed a period of slackness at high water, and after that a rapid outflow of the tide and a falling away of the level until the normal low-water conditions were restored. Some idea of the size and shallowness of the estuary could be obtained by considering the fact that 50 miles seawards, on a line drawn across the Gulf of Martaban between the outlet of the Rangoon river on the west bank and that of the Salween river on the east bank the width of the estuary was about 50 miles, but the general level of the bottom, still largely of a sandy character, was only about 15 feet below mean sea-level. The upper part of the estuary had never been charted. The rise of spring tide in the Gulf of Martaban was only about 8 feet above mean sea-level, whereas the rise in the river-channel beneath the Sittang bridge was between 20 and 40 feet. Those were conditions which gave rise to the famous Sittang tidal bore.

Between 1905 and 1928 a great change had occurred in the course of the estuary between the village of Kyaikkatha and the Sittang railway-bridge. Before 1905 the tidal channel between the bridge and the village had a course 50 miles in length, whereas it was now 10 miles. The change had come about through the short-circuiting of a great loop by a cut-off at the neck of the loop, called the Alok Cut. The Alok Cut had been a mere trickle when it had first appeared in 1905. It had attained a width of 500 feet with a depth of 30 feet in 1910, and in 192- it had grown to ½ mile in width, whilst in 1928 it had become 3 miles wide.

There was reason to believe that the short new channel was somewha

deeper than the old winding channel used to be, and there had certainly been a definite scouring action deepening the channel beneath the bridge, whilst in addition the level of the water at the bridge was lower than it used to be at corresponding states of the tide and fresh-water outflow.

Owing to the remoteness of the Sittang estuary from civilization there had unfortunately been no close and accurate records kept by reliable observers of what actually happened from year to year, and it had been necessary to piece together fragments of local information and records obtained from various sources, both European and native. The occurrence of the bore beneath the bridge being a somewhat spectacular affair, it had naturally left its mark in the memories of the local residents to a greater extent than changes in water-level and changes in position, and it had therefore been possible to gather fairly reliable information regarding the bore. There was no doubt that the bore used to appear underneath the bridge on spring tides between the years 1900 and 1913; that was to say, both before the formation of the Alok Cut and also during the early years before the Alok Cut had grown large enough to exercise an important influence. The bore did not appear in 1914 and 1915, but it reappeared in 1916. It became violent in 1924, running on one occasion beneath the bridge as a solid wall of water at least 10 feet high, and the action of the bore was observed to extend 8 miles upstream of the bridge in 1926. In 1928, the Alok Cut being fully established, the downstream mouth of the old channel had become entirely silted up and the bore beneath the bridge was not very noticeable. In 1930, however, it was again observed to be running more strongly, and on some occasions a bore-wave about 6 feet high was then observed beneath the bridge. What was so interesting from the evidence of the case was that before the Alok Cut formed, and in spite of the higher level of fresh water then obtaining under the bridge, the tidal wave used to be able to make its way up the long winding channel and to appear as a bore-wave beneath the Sittang bridge just as it did at the present time. The size of the bore-wave was not perhaps so large then as now, but the actual level of the crest above mean sea-level was very nearly the same.

The observations were not conclusive evidence of the importance of any particular factor, but they tended to indicate that a long tortuous channel was capable of transmitting a tidal wave of the same amplitude that a much shorter and more direct channel could transmit, a conclusion which ran parallel to Professor Gibson's theory that a 40-per-cent. restriction across a tidal channel might produce very little damping-out effect upon the tidal wave.

The Author, in reply, pointed out that if in *Fig. 9* (p. 496) additional points had been plotted showing the levels at the upper end of the Severn model—at Gloucester—the completed profiles would have shown the same phenomenon as in that of the symmetrical estuary, namely that in every profile except one the water-level at the head of the estuary was higher than

at the mouth. If, however, similar profiles were plotted for the symmetrica model modified by the presence of a 60-per-cent. obstruction, they wer essentially different from those from the unobstructed estuary, those o the flood showing lower levels at the top of the estuary than at the mout and those on the ebb showing the reverse. The form of the profiles wa bound to depend essentially on the form and length of the estuary, and o the relationship of its period of free oscillation to the tidal period.

It would be difficult to estimate the effect of the presence of bridge piers on the volume of afflux with any great accuracy, but in the case o the proposed bridge over the Severn with its 8-per-cent. obstruction of th channel, the effect on the tidal levels was so extremely small—only a smal fraction of an inch—that the effect on the volume passing would be almos infinitesimally small. In the case of a 40-per cent. obstruction it woul only amount to some 2 or 3 per cent. of the normal inflow and outflow.

The information regarding the tidal observations on the river Tyn was interesting as showing that, in spite of the obstructions afforded b the piers of the many bridges between the sea and Ryton, the level of hig water at the latter place was appreciably higher than at the sea, whils the level of low water was sensibly the same at both points. It would b of great interest if corresponding figures could be given for the river befor the bridges were built.

The description of the tidal phenomena in the Sittang estuary, and o the changes accompanying a straightening and shortening of the channel would tend to confirm the conclusions drawn from the Authors' experi ments, namely, that improving, from an engineering point of view, a estuary, did not necessarily give an increased inflow and outflow to th upper estuary, but might actually have the opposite effect.

Paper No. 5126.

"Some Experiments on Locomotive Springs, with Reference to Bridge Impact-Allowances." †

By William Edward Gelson, M.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Mr. C. W. Clarke, of Bombay, observed that the impact upon the rail from the driving wheel of a steam locomotive at any instant might be expressed by the general equation

$$Z=(R+W)+N+H+ky-W\ddot{x}/g$$

where R denoted the static loading on the bearing spring, W the weight of the wheel plus half the axle, N the thrust due to the slidebar-effect, H the dynamic augment due to counterbalancing, k the rigidity of the bearing spring, y the deflexion of the bearing spring from the mean position, and x the amplitude of oscillation of the wheel-centre, the values of x and y being taken algebraically. It would appear that the investigations had been conducted in order to determine the values of ky in the general equation.

In a locomotive, unless $H>(R+W)$, as a first approximation, the wheel could not lift, in which case the values of y were unaffected by H.

In the case of the I.R.S. "pacific" type locomotives, for $H>(R+W)$ the frequency of the coupled wheels had to exceed 10·7 revolutions per second, which was equivalent to a track-speed of over 140 miles per hour. It had been shown that, whilst the sinking of a rail started some six or seven sleepers ahead of the wheel, the trajectory of the centre of the wheel between rail-joints was practically a straight line. When the wheel struck a rail-joint, even at medium speeds, the values of $\ddot{x}$ in the general equation were certainly very much higher than those given by frequencies of rotation of up to 5 revolutions per second, as conducted by the Author.

The experiments conducted on the forced oscillations produced by rotating weights had no direct practical application in determining either the values of ky or the values of $\ddot{x}$ in the general equation.

The impact due to the effect of rail-joints would lead to the occurrence of "sub-impacts" as demonstrated by Mr. R. N. Arnold *, and it would be necessary to calculate the "statical equivalence."

† Journal Inst. C.E., vol. 8 (1937–38), p. 295 (March 1938).

* "Impact Stresses in a Freely Supported Beam." Proc. Inst.Mech.E., vol. 137 (1938), p. 217.

The frequency of rolling of a locomotive appeared to be independe of speed, and was fairly constant for any given design; it was abo 1·1 second for a complete oscillation in the case of the I.R.S. class X locomotives. It would seem, therefore, that if, in the experiments co ducted to determine the free oscillations produced by the cradle a kentledge, the cradle and kentledge had been so designed and pivot as to have a period of oscillation about the fulcrum of about 1·1 secon and with the static load on the specimen corresponding to the static loa on the locomotive bearing-spring, actual conditions could have bee reproduced enabling the values of ky in the general equation to be dete mined.

The arrangement shown in Figs. 1, Plate 1 (facing p. 304 §) for co ducting pulsating-load tests, corresponded more to the conditions lateral spring-control in a bogie. The static-load–deflexion tests showe that the frictional effect for used springs varied on an average from 1 per cent. to 61 per cent., so that any experiment using a coil spring havi the same calculated stiffness might produce errors many times great than the errors of observation.

In the pulsating-load tests, if instead of the coil spring a secon laminated bearing spring placed in an inverted position under the beari H (Figs. 1, Plate 1 (facing p. 304 §)) had been used, the effect of laminated-control-spring bogie would have been reproduced. The lengt of the shaft S and the balance-weights would have to be calculated t produce a dynamic augment equivalent to the couple causing nosin with the frequency of the dynamic augment corresponding to that nosing. The frequency of nosing varied with the speed, but frequenci of from 0·5 per second at slow speeds to 1·3 per second at high spee would cover the range for I.R.S. "pacific" type locomotives.

Another modification of Figs. 1, Plate 1 (facing p. 304 §) was suggeste If instead of the coil spring between the housing H and main frame a strut were substituted to bear against the underside of the housing H and then to pass clear of the main frame G and to rest on a rail mounte on sleepers (the rail-track being clear of the main frame G), the dampi effect due to the resilience of the track on the blow produced by th dynamic augment could be determined. That could be done either b using Fereday-Palmer optical strain-recorders to determine the deflexi of the underside of the rail, or by fitting a suitable extensometer to th calibrated strut between the housing H and the rail.

It would be a simple mechanical construction to combine the arrang ments shown in Figs. 1 and 2, Plate 1 (facing p. 304 §), so that the effe of rolling on the bearing springs could be reproduced by the oscillati cradle and kentledge acting on top of the bearing spring, and with th

§ Page numbers so marked refer to the Paper. (Footnote (†), p. 501.)—Sec. Ins C.E.

pulsating load produced by the rotating weights (representing the dynamic augment) acting on the housing H at the same time.

An extensometer fitted to the strut between the housing and the rail would show the combined effects of functions ky and H on the rail.

The Author, in reply, would emphasize that the object of his investigation was to enable a bridge-designer to calculate impact-allowances in medium-span railway-bridges with more certainty than had been possible heretofore. In such bridges, the fundamental natural frequency could synchronize with locomotive hammer-blow pulsations at the highest train-speeds, and considerable oscillation could occur with the structure and the unsprung mass oscillating in unison and out of phase with the sprung mass. Steam locomotives were still being constructed which caused considerable hammer-blow, and the extent to which such oscillations were possible largely depended upon spring and other damping forces.

The Bridge Stress Committee in their report of October 1928, took the spring-friction force to be constant in magnitude and alternating in sign, with a periodicity equal to that of the hammer-blow. Only the primary harmonic component of that alternating force was found to be of practical importance, and its phase-relationship was chosen so that the force was zero at instants of zero velocity of spring-movement. The spring-friction force in fact led the spring displacement by a quarter period.

The present investigation showed that the friction-force and its phase-relationship with spring-movement both depended upon the amplitude of the oscillation. That called for a little more work in calculating the bridge-oscillation, as a "trial and error" process had to be followed.

The Author had shown that spring-friction was negligible for small oscillations up to about $\pm$ 0·04 inch, and those limits were somewhat greater when the spring was assembled in the locomotive, on account of the flexibility of hangers, etc. The extra deflexion at properly maintained rail-joints came within the limit of negligible spring-friction, and therefore the absence of any factor for spring-friction in the formula given by Mr. Clarke for rail-impact from a driving wheel called for no comment, at least where permanent-way maintenance was good.

After examining a large number of rail-joint deflexion- and stress-records, the Author was able to state that the phenomenon of sub-impacts did not occur under any ordinary rail-joint conditions, which were essentially different from those in a beam subjected to a falling-weight test.

Many experiments had been carried out in the testing machine which had not been described, because they had no bearing upon the question of railway-bridge impact. Those experiments included fatigue-tests upon various standard and special types of laminated springs. Such springs were, in fact, tested in pairs as had been suggested by Mr. Clarke, but in that case the friction was so great that the limit of power of the

machine was reached before sufficient amplitude of oscillation had been attained.

The Author did not agree that Mr. Clarke's proposal would reproduce the conditions of a laminated-control-spring bogie. In such a bogie, the effective stiffness was usually half that for one of the springs, whereas in a arrangements used in the Author's machine for pulsating-load tests the sum of the stiffnesses of the springs had to be taken to give the effective stiffness.

CORRESPONDENCE
ON PAPERS PUBLISHED IN
APRIL 1938 JOURNAL.

Papers Nos. 5164, 5163, and 5165.

"The Galloway Hydro-Electric Development, with Special Reference to the Constructional Works." †

By William Hudson, B.Sc. (Eng.), and John Kenneth Hunter, B.Sc. (Eng.), MM. Inst. C.E.

"The Galloway Hydro-Electric Development, with Special Reference to the Mechanical and Electrical Plant." *

By William Hawthorne, B.E., M. Inst. C.E., and Frederick Herbert Williams, B.Sc. (Tech.), Assoc. M. Inst. C.E.

and

"The Galloway Hydro-Electric Development, with Special Reference to its Interconnexion with the Grid." ‡

By Reginald William Mountain, B.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Mr. H. H. Dare, of Roseville, N.S.W., observed that the explosive used for excavation was stated to have been No. 2 Polar Ammon gelignite. In some of the more recent dams in Australia a different type of explosive, with less shattering effect, had been preferred for dam-foundations. At the Stanley River dam (for a reservoir with a capacity of 725,000 acre-feet) which was at present being constructed on porphyry rock in Queensland, the resident engineer, Mr. Glenister Sheil, stated that Ligdyn, a 40-per-cent. nitro-glycerine explosive containing some 23 per cent. of sodium nitrate, was being used with satisfactory results. He reported that "at first glance this explosive would appear soluble and, therefore, unsuitable for wet holes, but, by leaving the wrappers on, we have not to

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† Journal Inst. C.E., vol. 8 (1937–38), p. 323 (April 1938).
* *Ibid.*, p. 376.
‡ *Ibid.*, p. 407.

date lost a hole or had a misfire. It appears economical, and does not knock the ground about."

The scientific methods of grouting adopted in the Galloway scheme represented a distinct advance on earlier practice. In the first place the use of thin 4 : 1 grout was bound to permit greater penetration than the 1 : 1 mixture frequently used in the past. The grouting of the holes in stages, the top 10 feet or so being done first so as to consolidate the upper layers of rock, and the re-grouting of the holes in the manner described in Messrs. Hudson's and Hunter's Paper, should give satisfactory results. Some additional information regarding typical grout-holes would be of interest, stating the quantity of cement injected in the primary and subsequent grouting-operations, exclusive of that required to fill the hole.

The extensive grouting carried out would, no doubt, provide a deep curtain more or less impervious to water below the upstream face of each dam, but, even so, it might be that in time some water would pass the curtain, and in such case it would appear desirable that the water should be led by drains to the downstream face. That matter was not referred to in the Paper.

One most interesting feature was the description of the methods adopted for obtaining a bond between the base of the dams and the foundation-rock, and watertightness in the construction-joints. With reference to the former, it was not clear why the cement grout, originally used with satisfactory results, had been abandoned in favour of 1½ inch of mortar, which seemed somewhat thick; nor why, under the final method, the rock was not coated with grout before the bonding layer of specially rich concrete was spread, thus providing, at small cost, an additional factor of security. In the construction-joints it was noted that the skin of the set concrete was entirely removed before placing the layer of special concrete; also that, to improve the shearing strength along the joints, the top surface of the concrete was rebated, or stepped upwards towards the downstream face. In spite of those wise precautions it was his view that the construction-joints were bound to remain a source of weakness, and that, however carefully the placing of fresh concrete against set concrete were done, there still would be a tendency for water to percolate along the joints.

So far as he was aware, information concerning the internal condition of any of the great dams in the world was very limited, and it was suggested that valuable data regarding the nature of the concrete and the percolation of water into the heart of the dam would be obtained if holes were drilled in some of the structures by means of modern calyx core drills. Those machines, which were being used in America for examining rock-foundations for dams, drilled holes up to 3 feet in diameter, thus allowing visual inspection of the sides of the hole.

At the Stanley River dam, referred to above, a series of percolation-

tests was being carried out under the direction of the chief engineer, Mr. W. H. R. Nimmo, and himself, with the idea of obtaining information concerning the percolation along construction-joints, under various conditions of treatment. Those tests, which were still in progress, were being made under a 100-foot head, and included investigation of the relative percolation along plain and rebated joints; along joints made by wetting only, or by covering with mortar about $\frac{1}{8}$ inch thick, before placing new concrete; and along joints using strips of metal as water-stops near the upstream face. So far, the metal strips appeared to give the best result, and consideration was being given to embodying copper strips, $\frac{3}{64}$ inch thick, and 6 inches or 8 inches wide, in all horizontal joints in the structure, set about 2 feet back from the face, the distance being dependent upon the space required for proper vibration of the face-concrete.

It was stated that the dams were built in bays about 50 feet in length, separated by closing spaces about 5 feet wide, which were subsequently filled with concrete after most of the shrinkage in adjacent monoliths had taken place. Although that method had been used in other dams, it was open to the objection that the shrinkage of the 5-foot closing strips might lead to the formation of two cracks instead of one; why was that method preferred to the more usual one of providing a suitably-shaped copper seal near the upstream face of the joints, without leaving any space between the monoliths? It would be of interest also to learn the results of experience with the grouting of the joints in the arch dams under pressure, after the 5-foot strips had been filled with concrete.

Mr. M. B. Duff had been especially interested in Messrs. Hudson's and Hunter's Paper, as he had had the privilege of assisting the Promoters when their Bill had been before the Committees of Parliament in 1929, more particularly with regard to the yield of the Loch Doon area and the compensation-water to be sent down the river Doon. Messrs. Hunter and Hudson stated that the amount of compensation which had to be given was equal to one-third of the average rainfall, which, as they said, was certainly a high figure. The circumstances were, however, unusual and complicated. The loch had been already raised and simple arrangements had been made for fish to enter. He remembered being present with the late Mr. R. C. Reid, M. Inst. C.E., to whom he was a pupil, at a meeting with the late Marquis of Ailsa, as far back as about 1885, with regard to alterations of those arrangements. Later, attempts had been made to give a minimum flow to the river Doon of 65 million gallons per day, but it was shown that the storage in the loch above the level of the old sluices could not maintain that flow in a dry season. The opposition of the mill-owners was also based on the supposed flow mentioned above. There was also the plea of the Ayrshire County Council that the diversion of the flood-water from the river Doon to the

Dee would necessitate the provision of sewage-purification works for Dalmellington and other places draining to the river Doon at an earlier date than would otherwise have been necessary. A reasonable money-payment was made towards settlement of that plea. Having regard to all the circumstances, the amount of compensation-water given might not be wondered at, and should not form a precedent in connexion with the ordinary case of taking water from an area not operated upon and not diverting it to another drainage-area.

The figures of rainfall, run-off and loss on p. 330 § were interesting, and showed how difficult it was to fix any definite amount for loss by evaporation. For instance, the loss in 1933 was 11·7 inches on a 49·6-inch rainfall, and 11·3 inches in 1934 on a 69·5-inch rainfall, the difference being no doubt due to the time of year when most of the rain fell. The average loss over the 6 years from 1930 to 1935 was 13·7 inches, confirming that the water engineers of a former generation were not far wrong in assuming a loss of from 12 to 16 inches for lower- and higher-rainfall areas.

He was greatly interested in the experiments made to ascertain the effect on smolts in passing through the turbines at Tongland through a pressure varying from atmospheric up to 45 lb. per square inch and back to atmospheric in less than half a minute, and he was pleased to know that actual experience had been satisfactory. The designs of all the fish-passes appeared to be excellent, and he hoped that they had proved themselves quite satisfactory in practice.

The figures on p. 362 § for the excess concrete for the lining of the Doon tunnel appeared to be very large, and he wondered if that were on account of bringing the finished tunnel to the exact specified size. To have left it larger at certain places would not appear to have mattered, but probably the reason was that the rock was so irregular after blasting that the extra concrete was rendered necessary.

Messrs. H. S. Hvistendahl and **G. Gianella**, of Baden, Switzerland, observed that, compared with usual Swiss hydro-electric practice, the Galloway plants presented a number of departures.

Referring to the Paper of Messrs. Hudson and Hunter, it was stated in the description of the Glenlee high-pressure pipe-line that electric welding had been adopted for the longitudinal seams, which were welded at the manufacturers' works, but that riveting was employed for the circumferential joints made at the site. In view of the fact that the stress in the circumferential joint was only half that in a longitudinal seam, and was still less when the effect of anchoring was taken into account, it would be interesting to know why welding was not considered suitable for the circumferential joints. Modern Swiss practice tended more and more to the exclusive use of welding, both in the manufacturers'

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§ Page numbers so marked refer to the Papers. (Footnotes (†, *, and ‡), p. 505.)—Sec. Inst. C.E.

works and at the site. In certain cases the spiral form of longitudinal seam had been employed for the lower part of high-pressure pipe-lines, to reduce the stress in the weld with a given thickness of plate; as, however, Continental experience, which was confirmed by that at Glenlee, had shown that for that class of work the efficiency of the joint could be taken as practically 100 per cent., the usual straight longitudinal seam was being generally reverted to.

Frequently, where the plate-thickness was 1 inch or more, the weld-portion was stress-relieved by local heating, and as manufacturers were beginning to instal X-ray equipment, there was a tendency to specify X-ray examination of a number of specimen strakes (usually 10 per cent.) selected at random. In other cases, the usual search for leaks by means of hammering during the hydraulic-pressure test was considered sufficient.

In regard to the material, the practice was to specify not only the ultimate and working stresses, but also the elongation, great importance being attached to ensuring a high value, namely about 25 per cent. for steels having an ultimate strength of from 50,000 to 60,000 lb. per square inch, which were usually employed, and about 23 per cent. for the higher-tensile-strength steels having an ultimate strength of from 60,000 to 70,000 lb. per square inch, which were occasionally used for the lower portions of the pipe-line. Frequently, the minimum Izod-test figure was also specified. The corrosion-allowance of $\frac{1}{8}$ inch was twice as much as that admitted on the Continent, but that might be due to the Galloway atmosphere being considered more corrosive than that of the European alpine districts. What was the length of the strakes, was the bifurcating branch-piece of cast or of welded design, and was it stress-relieved?

In the Paper of Messrs. Hawthorne and Williams it was mentioned that the Glenlee alternators were of the usual three-bearing design, but that the machines in the other stations running at lower speed were of the umbrella type. Although common in the United States, the umbrella-type alternator had not found favour on the Continent, partly because there was some doubt as to the tendency to vibration with only two bearings, and partly because of difficulty in performing the overspeed tests at the alternator-maker's works. Would the experience obtained at the Galloway plants cause the Authors to recommend the use of umbrella-type machines for further installations?

The general use of closed-circuit air-cooling for all machines was an innovation, in so far as only in isolated instances, where the noise question was of importance, had closed-circuit cooling-systems been employed in Continental plants.

It might be of interest to mention an ingenious arrangement employed at Tremorgio for damping the sound-waves propagated along the outlet duct of the pelton turbines. The device consisted simply of a water-curtain, sealing the space above the water-surface where the duct changed to an open channel. The water-curtain, for which the outlet water from

the air-cooler was used, had proved very effective. The main difficulty of the closed-circuit cooling-system was the cooling-water supply, especially in the case of high-head stations, where the amount of water taken by the cooler was an appreciable fraction of the total, and represented a serious loss of units; further, that energy had to be dissipated by the reducing valve, entailing a correspondingly high rate of wear of the valve. Had the reducing valves required a large amount of maintenance? Several important Swiss stations, notably Ryburg-Schwörstadt, had been designed with provision for the installation of closed-circuit air-coolers at a later date, having regard to the possibility of trouble caused by occasional swarms of flying insects during the summer season, but the precaution appeared to have been unnecessary.

The surge-towers were stated to be designed with storage-capacity to allow for a 100-per-cent. load being thrown on suddenly, as well as being thrown off. In regard to the condition of load-reduction, it would be interesting to know if the governing gear provided for the momentary diversion of the water-flow, or whether the only compensation was afforded by the surge-tower.

Concerning the devices protecting the sets against over-speed and lubrication-failure, what reserve source of energy was used for operating the governor? Was underspeed protection employed throughout or only at the unattended stations? In view of the relatively frequent starting and stopping, had brakes been provided to bring the machines quickly to rest when the speed had dropped below that at which an adequate lubricating oil-film was maintained in the thrust-bearing? Were the governors driven by belts, gearing, or by electric motors supplied from main or pilot generators?

Test-ponds represented an appreciable cost-item, and for that reason Continental engineers usually thought twice before installing them. They had, however, definite advantages, as they enabled the machines not only to be tested for efficiency on a steady load, but also enabled the sets to be subjected to governing tests without the steadying effect of the system-frequency. Experience had shown that governor-instability was more readily seen when the set was operated independently, and that a unit which governed satisfactorily alone would generally run satisfactorily when tied-in to the system-frequency.

The generating sets at Carsfad and Earlstoun appeared to be identical in all respects, operating under practically identical conditions, and it was, therefore, surprising that the measured efficiencies, according to Table V (p. 336 §) should differ by as much as 3½ per cent. Allowing for the alternator-losses, the overall value of 90·5 per cent. measured for the Earlstoun sets would suggest an efficiency of 94 per cent. for the water-wheel, which was more than would be expected for a machine of the type

§ *Ibid.*

and size concerned. The discrepancy appeared to be due to errors of measurement. Could such errors be attributed to the use of the salt-velocity method under conditions for which it was not suitable ?

Mr. Mountain's Paper very rightly stressed the need for re-examining water-power resources in respect of their suitability for economical generation of peak-load power. *Fig. 46* (p. 410 §) showed for the Tongland plant three generating sets coupled to the Grid through two 30,000-kilovolt-ampere transformers. The arrangement of three generating units with two transformers was somewhat unusual, because if conditions justified the splitting-up of the generating capacity into three units, presumably with a view to operating them as nearly as possible at maximum efficiency in spite of varying water-flow, then it was generally considered worth while to follow the same practice for the transformers.

Mr. W. H. R. Nimmo, of Brisbane, agreed with the opinion expressed by Mr. James Williamson (p. 433 §) that in estimating the mean rainfall over the catchment, the area represented by each rain-gauge should be taken into account. In a sparsely-settled country rain-gauges were usually widely distributed and were mostly located in the valleys, and he had found it necessary, in investigating the yield of areas comprising mountains and valleys, where there were steep rainfall-gradients, to modify the results obtained from the rain-gauges to take into account the variation of rainfall with elevation.

The Tables on pp. 330 § and 433 § showed that the average loss deduced by Mr. Williamson was 1·4 inch less than that given by the Authors. The 6-year period of record was insufficient to establish the minimum annual rainfall, and should drier years than those so far experienced occur, then the reduction in the loss might be important. The difference between the loss from the Severn and Dee catchments, referred to on p. 447 §, might be due to the greater area and greater range of rainfall within the former area. A flood of 28,000 cusecs from 144 square miles was rare, but in his opinion considerably larger floods were possible.

His experience confirmed that of Messrs. Hudson and Hunter that too little attention was given to the shape of the particles of concrete aggregate (p. 365 §). Flaky stone produced a less workable mix, which required more water, with consequent reduction in strength. The concrete aggregate for a dam which was now being constructed under Mr. Nimmo's direction was derived from a porphyrite which was of a flaky nature, but by the use of vibrators it was found practicable to consolidate concrete having a slump of $\frac{3}{4}$ inch. Although the inclusion of stone dust decreased the strength of concrete, it might perhaps be found also to decrease the permeability ; in regions where the water might be aggressive to cement, that advantage might outweigh some reduction in strength.

§ *Ibid.*

Mr. Guy Richards thought that it might be of interest to give some details of the steel pipe-line at Glenlee. Pipes were delivered to the site by road in 24-foot lengths, fabricated from three 8-foot strakes with plain butt-welded circumferential joints; all field-joints were riveted butt joints with cover-plates. The pipe-lengths were transported into position by bogies on a 3-foot-gauge railway running parallel to the pipe-line, were rolled off the bogies, and were manœuvered into line. The general procedure of erection was to set, in turn, the vertical bends at the anchor-blocks (Figs. 31, Plate 3, facing p. 454 §) accurately for line and level, and partially to concrete them in, after which the pipes were erected to the next bend, and, as soon as that was set, the intervening pipes were jacked into line and riveted. It was interesting to note that the two circumferential welds in each length of pipe caused a shrinkage of approximately $\frac{1}{8}$ inch in the 24-foot length. Insignificant as that might appear, it necessitated some adjustment at the anchor-blocks and expansion-joints to counteract it.

Whilst each length of pipe was hydraulically tested in the shops, the Contractor decided to test the field-joints before testing the completed pipe-line, by drilling small holes in the cover-straps and injecting paraffin under pressure. In that way any defect in the internal sealing-weld at those joints could be detected and rectified with a minimum of delay and trouble. The final test on the completed pipe-line showed all shop-welds to be absolutely tight; the only leakage which gave trouble occurred at the crutch of the bifurcation, which was rectified by additional welding.

The connexion of the bifurcation to the two 6-foot-diameter butterfly valves immediately downstream presented an interesting problem. The design allowed for a $2\frac{3}{4}$-inch gap between the valve- and bifurcation-flanges, which was to be filled by a special packing made to a template after erection, as a metal-to-metal contact was required. Whilst it was realized that those packings would in all probability require to be slightly tapered, since the valves had to take their setting from the 6-foot-diameter pipes, it was found that the heavy bifurcation-flanges had warped during welding to a maximum of $\frac{3}{16}$ inch out of a true plane. The Contractors ingeniously overcame that difficulty by making the packing of two rings spot-welded together to give the correct taper; after insertion, small wedges were driven between the rings until perfect contact was obtained on both flanges, and then the gap in the packing was welded solid all round.

Some difficulty was experienced in setting the base-plates for the sliding saddles which supported the pipe-line between the anchor-blocks. In order to obtain a perfect bearing, the design allowed for those base-plates to be temporarily bolted up to the saddle bearing-plates while they were grouted in to the concrete pier, the pipe-line, although completed,

§ *Ibid.*

still being supported on packing. It was found, however, that the expansion and contraction of the pipe-line during the setting period of the grout dragged the base-plates and loosened the grout. The method finally adopted was to wedge the base-plates off the piers up to the saddles so that sliding could take place. The base-plates were surfaced with phosphor-bronze and coated with graphite grease.

From experience gained on that pipe-line it was evident that shop-welded strakes in conjunction with riveted field-joints gave a most satisfactory and economical result. It was considered that welded field-joints would not be practicable with pipes of the diameter and plate-thickness employed, due to the difficulty which would be experienced in holding the ends of pipes to a true circle during welding, together with the fact that it would be impossible to line the pipes and at the same time to maintain a constant butt-gap. It was also apparent that welding introduced minor distortions which were difficult to anticipate in the shop; the Contractor, however, had maintained a high standard of workmanship throughout.

Dr. E. G. Ritchie observed that the peak-load problem in power-generating stations was one of growing importance. In 1925 the aggregate maximum demand in Great Britain was about $2\frac{1}{4}$ million kilowatts, whilst by 1935 it had grown to nearly 5 million kilowatts. Over the same period the gross capacity of reserve plant, expressed as a percentage of the aggregate maximum demand, fell from about 80 per cent. to about 33 per cent., with a further drop to 25 per cent. in 1936 and to 20 per cent. by the end of 1937. As the demand was at present increasing at the rate of approximately 1 million kilowatt per annum, the percentage of available standby was obviously approaching the lowest permissible level.

Having regard to the fact that development of the base-load stations was nearing completion, attention would have to be directed to the development of the lower-load-factor stations. An important consideration was that, although the load-factor on the system as a whole had improved, and was likely to continue to improve, there was some evidence to show that the load-factor of at least the top 10 per cent. of the aggregate-load curve tended to diminish. The problem of the low-load-factor stations (embracing, as it did, the peak-load problem itself), was, therefore, assuming an importance that it did not have 3 or 4 years ago, when semi-obsolete plant in abundance was available for short-period service.

In peak-load plant, perhaps the most important consideration was availability. In the Galloway scheme 33 per cent. of the total capacity could be developed within 5 minutes, and 77 per cent. within 15 minutes, whilst the full capacity of the system was not available under 25 minutes. That did not compare favourably with the performance of modern boiler-plant, which could be brought up from a nominal rating to full rating in from 6 to 8 minutes. With specially-designed steam-raising equipment the time taken to get the boilers fully under way was even less, and steam

plants were in regular operation in which a full head of steam was attained from a live banked condition in from 3 to 4 minutes. Where steam accumulators were installed, a change in load over the full range of capacity of the plant could be met in less than 5 seconds with generating units as large as 25,000 kilowatts. If, therefore, the time taken to bring the Galloway plant up to full load was characteristic of peak-load hydro-electric plant, the lower availability as compared with specially-designed steam generating plant was a definite disadvantage.

In considering peak-load plant, low capital cost was of fundamental importance, and in that connexion the Galloway scheme was bound to be regarded as costly, the price being about £31 per kilowatt of operating capacity, including the cost of interconnexion to the Grid. The total cost per unit generated by the Galloway plant compared favourably with the corresponding figure for a modern steam-station operating with the same load-factor. A load-factor of 20 per cent. could not, however, be regarded as representing true peak-load conditions, and, moreover, steam generating plant specially designed for peak-load service would be much less costly than that proper to a high-load-factor station. With load-factors of the order of from 5 to 10 per cent. a comparison should definitely favour steam generating plant specially designed for that duty. In that connexion steam-accumulation offered important possibilities.

In the Papers no indication was given of how either the total cost per kilowatt-hour on the Galloway scheme was computed, nor was the method of calculating the capital charges shown. A statement by the Authors on that point would be of very great value.

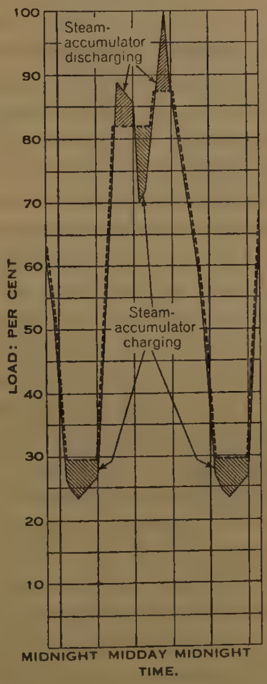
Fig. 57 showed the approximate shape of the aggregate-load curve throughout Great Britain during a mid-winter day, the load-factor for the whole system during that day being about 60 per cent. The top 30 per cent. of that in effect represented the peak-load problem, as the rapid drop in the demand during the mid-day break was as disconcerting to the system as were the peaks themselves.

If steam-accumulators were installed, an analysis of the diagram showed that the economical sub-division of the load would be to carry about the top 12½ per cent. of the load on stored steam. That would mean in effect that in an area where the demand totalled 500,000 kilowatts, about 62,500 kilowatts would be met by accumulated steam on a time-base of about 2½ hours. As the major peak was of triangular form, the steam-storage capacity to be provided per discharge would be equivalent to approximately 78,000 kilowatt-hours.

With steam-storage capacity installed to the extent of 12½ per cent. of the maximum demand, the equivalent boiler-firing line would be as shown dotted in *Fig. 57*, the accumulator-battery being charged during the night and during the mid-day break, and discharged to meet the morning and afternoon peak loads, as shown by the respective shaded areas. That would mean that, with the load-curve indicated, the boiler-capacity engaged

would be constant for about 5 hours during the night and about 10 hours during the day, with an increase from normal rating to about 7 per cent. overload rating over a period of about 4 hours in the afternoon, to cover the lower part of the major peak. All variations in load above and below the night and day firing-rate would be covered by the automatic charging and discharging of the steam-storage system. That would raise the boiler-house load-factor of the whole generating system from 60 per cent. to 70 per cent. Alternatively, if all load above the 50-per-cent.

Fig. 57.



level were allocated to a two-shift station, the boiler-house load-factor of that station would be increased from 35 per cent. to 50 per cent. by the installation of steam-accumulators.

In many stations which during the past few years had been developed into high-load-factor stations, semi-obsolete turbine plant was available. In such stations, steam-accumulators for peak-load service could profitably be installed, using the old turbines, modified, if necessary, to enable them to operate on accumulated steam. That would provide useful fly-wheel effect for the base-load boiler-plant, and would at the same time profitably absorb existing turbine equipment. Alternatively, existing two-shift stations might be equipped with steam-accumulator batteries to enable

them to deal with the peak-load demand for energy and to permit stations to be shut down which would otherwise be required only for short periods of time throughout the year.

The installations of steam-accumulators as suggested would reduce the operating costs on the system as a whole, as labour charges on the peak-load plant would be confined almost entirely to additional turbine-room attendance, and, whilst the heat-consumption per kilowatt-hour would be somewhat greater on accumulated steam than on live steam, that would only affect a negligible percentage of the total yearly demand ; further, in any case improvement in the boiler-house load-factor and stabilization of the live-steam pressure and temperature due to the elimination of peak-load firing should more than offset the slight increase in the heat-consumption for the generation of the peak-loads. On the basis of a triangular peak-load lasting 2½ hours, the total capital cost of steam-accumulators, including foundations and all building work, and including also a reasonable sum as representing possible modifications to the turbines used for peak-load service, worked out at approximately £4 10*s* per kilowatt of operating capacity. That was bound to compare favourably with the cost of meeting peak loads by any other means, especially having regard to the fact that a steam-accumulator came into contact only with distilled water and saturated steam. There was nothing to cause its deterioration, and its useful life was much longer than in any other item of power-station equipment. In addition, when charged, steam-storage plant offered the advantage of providing a reserve of energy almost instantly available.

Dr. R. A. Sutherland, of Hastings, Nebraska, was particularly interested in the relatively high specific speed obtained at the Tongland plant. The range of head was from 95 to 114 feet and the specific speed was 84 revolutions per minute. That was considerably above the commonly used American curve, which would give a specific speed of about 55 revolutions per minute for a head of 95 feet. In deciding on suitable speeds for turbines for three plants at present being built in Nebraska, two of which had a gross head of 113 feet, he had had occasion to consult a number of turbine-manufacturers, and he had been advised that a specific speed of about 55 revolutions per minute was as high as the manufacturers would care to bid on. The machines were of 13,000-h.p. capacity, and a specific speed such as had been used at the Tongland plant would have enabled the speed of the machines to have been raised probably to 225 revolutions per minute (60 cycles per second) instead of 180 revolutions per minute, with a consequent considerable saving in the cost of electrical machinery. He would be glad if the Authors could give some more details of those interesting turbines.

He was also glad to see the use of six arch dams on the one development. Those dams were amongst the first of their type in the British Isles.

Had the Authors considered the use of the differential surge-tank as

an alternative to the simple surge-tanks used, and if so why had they been discarded? The differential tank had become almost general in the United States for installations of any size requiring a surge-tank, and it seemed to him that a considerable saving in size of tank could have been made at the Tongland plant by the use of that type of tank.

Mr. Ronald Walker would like to refer to two aspects in connexion with the concrete-production, namely the gradation of aggregates, and controlled batching.

With regard to gradation of aggregates, he had no doubt that the importance of gradation of aggregates in relation to workability, density, and strength, of the concrete, had been fully appreciated. He held the view that the gradation of aggregates for concrete should be controlled within comparatively narrow limits[1], so as to ensure optimum workability, density, and strength, with minimum cement-content (and therefore minimum liability to temperature-effects). Further, he had been partly responsible for the evolution of plant for the automatic control of gradation. He would, therefore, be interested to learn what variations obtained in the sieve-analyses of the aggregates, and whether any information was available regarding the effects of such variations on the workability, density, and strength of the concrete.

He would welcome the Authors' observations regarding the contention that, with regard to the construction of large concrete structures for the retention of water, requirements could best be met by the use of aggregates conforming to such gradation-curves as would, in conjunction with the minimum content of cement (preferably coarsely ground), result in concretes of maximum workability and density, with minimum liability to be adversely affected by temperature-effects.

Applied in practice, the foregoing would mean, so far as gradation of aggregates was concerned, that the ideal gradation-curve would be found by experiments, and plant (which was now available) would be employed to ensure that all aggregates used would constantly and reliably conform thereto.

In regard to controlled batching, he would be interested to learn what steps were taken to ensure that the manual control of batching resulted constantly in the concrete produced being strictly in accordance with specification, and to learn, approximately, the cost of taking such steps. Batching-plant which operated automatically, and in which the proportions were guaranteed by mechanical interlock, being now available, he would like to have the Authors' views regarding the desirability of employing such plant.

Mr. Hunter, in reply, observed that he was interested to hear of the

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[1] See Prof. H. N. Walsh, "Aggregate Grading and Concrete Quality." Trans. Inst. C.E. Ireland, vol. lix (1932–33), p. 275.
—— "Aggregate Grading in Relation to Concrete Mix Design". *Ibid.*, vol. lxii (1935–36), p. 197.

favourable experience which had been obtained at the Stanley River dam from the use of the explosive Ligdyn. In Galloway, although the various works had been carried out by a number of different contractors, No. Polar Ammon gelignite had been used almost exclusively, and, so far as he knew, no serious attempt had been made to experiment with any other explosive.

Mr. H. H. Dare had asked for additional information with regard to the grouting operations. At Tongland arch dam, alternate holes 30 feet deep had been drilled and grouted, after which the intermediate holes had been drilled and grouted. In the initial stages of the injection, the grout consisted of 8 per cent. of cement by weight, thickened to a 60-per-cent. mixture for finishing-off. Seventy-two holes took an average of 48 cwt. of cement each, the maximum for one hole (two groutings) being 218 cwt. It should be mentioned that the figures given for the quantity of cement used included the grout required finally to seal the tubes.

Whilst the grouting which had been carried out on all the dam-foundations had apparently been very successful, it had been recognized that those operations could not be entirely depended upon to seal off all leakage through the rock or along the plane of contact between the dam and the rock. In the case of the arch dams any uplift arising from such leakage would have no appreciable effect on the stability of the structure. In the case of the gravity dams, provision for counteracting the effect of a reasonable amount of uplift had been made in the designs of the dam profile, and no under-drainage had in any case been provided.

Mr. Dare's references to the experiments being carried out at the Stanley River dam on the percolation of water along construction-joints were interesting. Messrs. Hudson and Hunter had found that, provided the contractors were conscientous in carrying out the precautions laid down by the engineers when depositing a fresh lift of concrete, a perfectly watertight joint could be relied upon even in the thin arch dams. The production of a watertight joint by ordinary methods called for unremitting care, however, and it occasionally happened as a result of carelessness that a defective joint would be revealed when the reservoir was filled.

Mr. Dare also referred to the grouting of the vertical construction joints between the bays of the arch dams. The result of the methods adopted and described on pp. 358 § *et seq.* had been entirely satisfactory, and no percolation of water along any of the closing-space joints had been observed in any of the arch dams. In the case of the gravity dams the upstream key in the face of the closing spaces had been painted, before filling-in with concrete, with a thick bitumastic compound, and that method had proved equally successful in sealing the vertical joints between bays.

With regard to the figures given in their Paper for the overbreak in the Doon–Deugh tunnels, to which Mr. Duff drew attention, they were mainly

§ *Ibid.*

to be attributed to the contractor's desire to cut down trimming expenses to a minimum. In the case of the Glenlee tunnel, which was carried out by another contractor, the main heading was driven "tight," resulting in relatively little overbreak but consequently increased trimming. It was a matter for argument which of the two methods resulted in the greatest overall speed and minimum expense.

Messrs. Hudson and Hunter were indebted to Messrs. Hvistandahl and Gianella for their contribution in connexion with high-pressure pipe-lines. It would be found that their queries in connexion with the welded pipe-line were answered by Mr. Guy Richards, who was the member of the Consulting Engineers' staff responsible for the field-work, and whose contribution formed a valuable addition to the subject of the Papers.

Messrs. Hawthorne and Williams, in reply to Messrs. Hvistendahl and Gianella, observed that there was so far no indication that the umbrella type of design caused vibration of the machines, and Messrs. Hawthorne and Williams would have no hesitation in using that design if the Galloway plant had to be repeated. The question of vibration with only two bearings was chiefly a matter of exact static and dynamic balance. The alternators had been over-speeded in the works, with the shaft vertical. A short stump shaft had been bolted to the coupling and a small steady-bearing had been provided. The distance between the two guide-bearings under test was less than between the two guide-bearings on site, so that the overspeed test had been carried out under much more severe conditions than in normal running. The closed air-circuit for the alternators and exciters had been adopted to prevent moisture condensing from the humid atmosphere on the windings of machines, which would frequently be standing for hours between periods of peak load. There was also no risk of a muddy deposit forming on the end windings from any dust that might be present in the atmosphere. Noise had been reduced by closing in the machines, and particular attention had been given to the stiffening and supporting of the cover-plates to prevent drumming. With regard to the reducing valves for the cooling water, maintenance on them had been negligible.

At Glenlee and Kendoon, where the pipe-line conditions demanded it, automatic pressure-relief valves operated by the turbine-governor were provided to divert the water-flow momentarily. Surge-towers were necessary at those two stations and also at Tongland. Three alternative drives were available for the governor-pendulums: belt-drive, direct mechanical drive from the turbine-shaft, and electrical drive by a synchronous motor fed from a potential transformer connected to the main generator-leads. The last-mentioned method was used for turbines of such dimensions that direct drive would be heavy and awkward to arrange. The main Galloway machines, however, were of a size that permitted simple and effective mechanical drive to be employed, whilst belt-drive

was adopted for the small auxiliary sets in Tongland and Glenlee stations. The mechanical drive comprised a vertical shaft driven by spur gears from the turbine-shaft, and driving through bevel gears a horizontal shaft carried in bearings to the actuator. A gap in that shaft was spanned by a shrouded stainless steel strip held in slots, which eliminated vibration and was self-aligning.

The operating oil was supplied to each servo-motor by a geared rotary pump with an idler-valve. The oil was fed into a pressure-receiver in which was a cushion of air, replenished either by an air snifting-valve on the suction-pipe of the pump, or from an air-compressor set. Bus-pipes interconnected the pumping sets which were of such capacity that, even if one set were out of action, there would be sufficient remaining capacity to operate all the servo-motors in the station. It was common practice to depend on the snifting-valve for replenishing the air, but the process was too slow for the frequent starting and stopping of peak-load operation. Air-compressors with automatic control-gear had accordingly been installed in duplicate at Tongland and singly at the other stations, where provision was also made for connecting in a portable compressor when the station compressor was being overhauled.

The governor-actuator contained emergency gear which closed the turbine in case of failure of the pendulum drive or the oil-pump drive, or if the speed dropped to half normal. An overspeed safety governor mounted on the turbine shaft was adjusted to trip at 32 per cent. above normal speed. It operated twin valves on the governor servo-motor through an oil-pressure relay, and caused the servo-motor to close the turbine-gates; at the same time the trip-gear, through an electrical relay, operated the control-valve of the main valve or gate.

A set of brakes bearing on the underside of each alternator-rotor, and operated by air-pressure at from 40 to 50 lb. per square inch through a solenoid-operated valve controlled from the machine indicator-panel on the turbine-room floor, could bring the machine to rest from full speed in 5 minutes. Safety switches were inserted in circuit with the solenoid to prevent the brakes from being applied until the governor had closed the turbine-gates and the main circuit-breaker of the machine had been opened. The brakes might be applied with the machines running at full speed, and the brake-blocks were of material such that the friction did not produce metallic dust which, by settling on the windings, might cause short circuits. The brake-cylinders could also be used with oil at high pressure to jack up the alternator-rotor. The brake-system was tested to a pressure of 3,000 lb. per square inch.

The three test-ponds had been installed in view of the inconvenience which would be caused to the Central Electricity Board by frequently throwing on and off load at the middle of a long transmission-line when governor-tests were made. They also enabled a set to be tested while the others were supplying power to the Grid.

The Earlstoun and Carsfad runners were of the same general design and dimensions. When they were designed a difference of head of from 6 to 8 per cent. had been expected between the two stations; that, together with the results of experiments carried out on models in their hydraulic testing station, had led the manufacturers to make slight differences in the shape and arrangement of the runner-blades for the two stations. The difference in efficiency determined on site was accounted for partly by that and partly by the fact that, the normal head at Earlstoun being slightly greater than that at Carsfad, there was a displacement of the gate–load curve. During the efficiency tests, for each loading of the machines the readings of electrical output were taken from four, five, and six electric meters, and those for waterflow from several "shots" of the salt-velocity method. Those readings were consistent among themselves, and since the test-conditions at Earlstoun and Carsfad were so nearly the same, the test-results at the two stations should be equally accurate. Taking the salt-velocity tests on all the stations, it could be said that the percentage deviation of the maximum from the mean of the readings of water-flow velocities with each quantity of water flowing was comparable with (and in the case of the longer conduits, less than) the percentage of the maximum deviation from the mean readings of the electric meters.

It was intended that the Galloway stations should run usually at full output. Estimates of the overall costs, taking into account excavation, buildings, plant, switchgear, generating and transformer losses, had come out less when the generating capacity was split up between two or three generating units and the transformer capacity was concentrated in one unit, than with one machine and transformer in each station.

Dr. Sutherland referred to the adoption of a specific speed of only 55 revolutions per minute for 13,000-h.p. machines under a head of 113 feet. That might be due either to a desire to flatten the efficiency-curve, to special conditions affecting the regain of head in the draught-tube, to the loading being partial for much of the time, or to the quality of the material used for the runners. The specific speed of 84 revolutions per minute adopted for Tongland had been used for similar machines that had been in satisfactory service for several years.

Differential surge-tanks had not been adopted because any saving in cost would not have compensated for the increase in the size of pipe-lines, the greater momentary speed-rise (WR^2) of alternators, etc., required to balance the choking effect of the stand-pipe on the governing of sets.

Dr. E. G. Ritchie's remarks were a contribution to the literature of another subject, and Messrs. Hawthorne and Williams felt that they could not deal adequately with the relative merits of hydro-electric stations and steam-accumulators for peak-load supply in a reply to the discussion on the present Papers. With regard to capital costs, however, they would point out that the Galloway system, with an annual load-factor of over 20 per cent., did not deal exclusively with the finer peaks of the load-curve of the

Grid; for that purpose sites were available where the capital and operating costs per kilowatt would be much less than at Galloway.

Mr. Mountain, in reply, wished to thank Messrs. Hvistendahl and Gianella for their support of his suggestion that a re-examination of water-power resources would be of value.

Dr. E. G. Ritchie asked for a statement of the way in which the total cost per kilowatt-hour of electricity generated in the Galloway scheme was computed. The total capital charges and working costs of the scheme were stated to be a little under £240,000 per annum, which, with an output of 180,000,000 units per annum, corresponded to a total cost of generation of electricity of 0·32*d.* per unit. It was stated that that did not allow for the cost of any additional equipment required for the interconnexion with the remainder of the Grid system.

The amount of £240,000 could be divided roughly in the following way: interest and sinking-fund payments, £185,000; rates, £15,000; maintenance, including a reserve for machine-renewals, £28,000; and operating expenses, £12,000. The operating and maintenance costs were an estimate of the average annual costs to be expected, and the costs to the present time had been below those estimates.

Paper No. 5170.

"Engineering Problems Associated with Clay, with Special Reference to Clay Slips." †

By Thomas Hardman Seaton, M. Inst. C.E.

Correspondence.

Mr. J. M. Lacey observed that one of the chief causes of slips in cuttings and erosion of cliffs was the surface-water at the back of the cutting or cliff. In that connexion he would like to draw attention to two Papers published by The Institution *.

Mr. A. R. Pollard thought that there seemed to be some doubt about the actual form assumed by the surface of a clay slip, and that further information on the matter was desirable.

Some years ago he had presented to The Institution a Paper entitled "Profiles of Earth Dams when Clay is partly or wholly used in the Embankment" ‡. In that Paper an attempt had been made to determine the changes of slope needed to ensure the stability of a high clay bank. That changing slope was referred to by Mr. Seaton (p. 497 §) and by Dr. Herbert Chatley (p. 487 §). According to the conclusions reached in Mr. Pollard's Paper ‡, a slope of 1 in 3 would be stable for a bank or cutting from 40 to 50 feet high where the clay was firm, as it would be in a well-drained bank or cutting. With wet clay the height would be 20 feet or even less.

Dr. Chatley, on p. 487 §, mentioned that the sliding surface was assumed to be circular, cycloidal or a logarithmic spiral. In his own Paper ‡ Mr. Pollard had calculated a curve of rupture, the shape being such that the horizontal thrust was a maximum, with the coefficient of friction decreasing as the height of the bank increased.

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† Journal Inst. C.E., vol. 8 (1937–38), p. 457 (April 1938).

* R. B. Stanton, "The Great Land-Slides on the Canadian Pacific Railway in British Columbia." Minutes of Proceedings Inst. C.E., vol. cxxxii (1897–98, Part 2), p. 1.

* J. M. Lacey, "Littoral Drift along the North-East Coast of Kent, and the Erosion of the Beltinge Cliffs near Herne Bay." Inst. C.E. Selected Engineering Paper No. 72. (1929.)

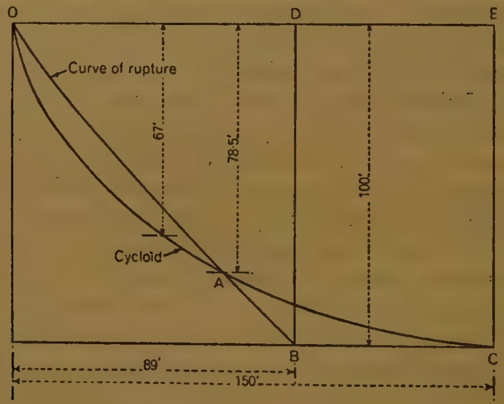
‡ Paper No. 4603. *Abstract published in* Sessional Notices Inst C.E., Session 1926–27, p. 98 (May 1927). [The MS. and illustrations may be seen in the Institution Library.—Sec. Inst. C.E.]

§ Page numbers so marked refer to the Paper. (Footnote (†), above.)—Sec. Inst. C E.

The curve of rupture and a cycloid were shown in *Fig. 15* for a bank 100 feet high made of clay that would not, by itself, stand at a greater height. The tops of the sliding surfaces at Folkestone Warren in 1915 and 1937 (*Figs. 13* and *14*, facing p. 477 §) seemed to resemble the curve of rupture much more closely than a cycloid.

The pressure against the surfaces DB and EC (*Fig. 15*) at various heights would be calculated by the methods used in his Paper ‡. The calculations showed that for heights up to about 19 feet the horizontal thrust from the cycloid was slightly in excess of that from the curve of rupture. Between heights of 19 and 67 feet the curve of rupture gave the greater horizontal thrust, but thereafter the pressure from the cycloid

Fig. 15.



was greater, which might be expected since, at the greater heights, the mass enclosed between the cycloid and the vertical line against which the pressure was calculated was much larger than the corresponding mass for the curve of rupture.

So far as calculations were concerned, the curve of rupture was much easier to work with than the cycloid, and could be safely used in preference to the latter when the height of the bank or the depth of the cutting did not exceed two-thirds of the limiting height of the clay.

Mr. E. F. Sykes, of Bundi, Rajputana, observed that, while working on the headworks of a Panjab canal, he had been concerned with hydraulic mortars; among other substances, he had made briquettes of clay. By "clay" was meant the alluvium thrown down by the rivers on the inside of their bends or in spill channels, and called in its early stages *Dal-dal* or "quicksand"; it was probably finely-ground rock from the ice in the back hills. Like Mr. Harold Berridge, he had found a remarkable

§ *Ibid.*
‡ *Loc. cit.*

strength in those briquettes, which like Mr. Berridge's, had sloughed when put in water. When, however, slaked lime had been mixed with the clay in varying proportions, the result had been very different. After a short period in damp sand, the briquettes could be cured in water in the usual way, with continually increasing strength. He had, in fact, made a hydraulic mortar. That suggested an entirely fresh approach to the problems with which the Paper dealt; namely, no longer to put up with the inconvenient properties of the material, but to convert the material into something else; that was to say, to convert clay into hydraulic mortar, which did not have the inconvenient properties of clay.

The appropriate method could only be determined by experiment, which, whether in the laboratory or the field, was extremely inexpensive. As a beginning he would suggest that in, say, a new bank or cutting, the formation-surface should have the lime puddled into it, in the shape of a turtle-back, and the surface of the slopes should be dealt with in a similar manner. If a cut-off were required, a trench might be back-filled with clay puddled with slaked lime.

The slaked lime used in his experiments was the nearly pure calcium hydrate that used to be known as "Miani lime."

Mr. F. L. D. Wooltorton, of Shwebo, Burma, thought that it was a great pity that the Author of the first Paper read before The Instituion for a considerable time having any bearing on soil-mechanics did not present his Paper in a more modern form. If the Author had adopted a more modern engineering definition for clay soils, his introductory remarks would not only have been more accurate, more interesting and their sequence more logical, but he would have found that his subject was far more extensive than he apparently imagined. There were many very troublesome clays which did not fall within his definitions.

The Author's general definition of clay was, if anything, misleading. There were, he felt sure, many clays in which plasticity was not of primary importance, and some in which plasticity was negligible, even for abnormal moisture-contents. The clays the Author designated as "laterite" would probably, in general, be non-plastic over very wide variations of moisture-content.

Plasticity as a predominant characteristic was associated with clay under one or two main conditions: (*a*) richness in inorganic material, and (*b*) having a clay fraction of a siliceous nature. Clays rich in sesquioxides showed little plasticity.

In dealing with the clay materials the Author did not appear to have differentiated between a clay soil and the clay fraction; between the primary non-colloidal clay-forming minerals such as felspar and pyroxenes, for example, and the secondary or fundamental colloidal crystalline minerals of the clay fraction, such as bentonite and halloysite.

Modern research as demonstrated by Messrs. Kelley, Dore and Brown,

and C. E. Marshall indicated that the Author was incorrect in describing those colloidal minerals as amorphous. The modern view was that they were crystalline minerals, usually in the form of a gel, having a definite crystalline lattice structure.

The modern view further stated that, although those colloidal minerals were often present together and in varying proportions, yet a clay in any soil group subjected to a definite and geologically-prolonged weathering-process would show a predominance of, or entirely, a single colloidal mineral as the end product of that system of weathering. The production of kaolin was merely a particular case.

He doubted if it were correct to describe laterite and loess as clays without further qualifications. Laterite was certainly not a clay, though lateritic soil was. One was the end product of silica weathering, producing a rock-hard conglomerate material; the other was a very early stage which might never end in the formation of laterite, as exemplified by some red soils classified as terra rosa. Loess in semi-arid regions was often of a sand texture, although prolonged weathering to a chernozem would produce a clay.

The statement that if an unstable clay were allowed to dry out it would again become stable (p. 461 §) should be accepted with considerable caution. Such a condition would depend upon a number of circumstances. In engineering, to permit a foundation to dry out was to court trouble. Examples were on record where foundations were artificially kept moist.

Dr. Stradling raised a very pertinent question regarding the obtaining of information about soil-mechanics. Early in 1937, whilst on leave, Mr. Wooltorton had discussed that question with a member of the Imperial Bureau of Soil Science. Later in the year he understood that the Bureau were undertaking the compilation of a bibliography for the subject. Unfortunately most of the available information was in a foreign language or had been published in America. In either case he knew from experience that it was extremely difficult to obtain any written information from sources available in England.

The Author, in reply, observed that Mr. Sykes recalled an experience in which he mixed lime with clay obtained from the bed of a river in India, and found that he could make briquettes from the mixture which, when cured in water, had greater strength than briquettes made from the clay alone: further, briquettes made from the mixture were able to withstand the action of water without sloughing; Mr. Sykes pointed out that the mixture of clay and slaked lime functioned as a hydraulic mortar. Based on that experience, he suggested a method of hardening the surface of banks or cuttings in clay by puddling in lime, and thereby rendering the banks impervious to the washing and disintegrating effect of drainage water. Such a proposal, if successful, would offer an attractive means of

§ *Ibid.*

hardening a freshly-cut surface in a clay bank, but the Author considered it doubtful whether conditions in actual practice were similar to those applying in the experimental work carried out by Mr. Sykes, and on which he based his proposal. It would be recognized that in the preparation of those briquettes from the lime-clay mixture the curing and ageing took place under water, and therefore in the absence of carbon dioxide, which would be present if the mixture were exposed to the air; the carbonation of the lime would be prevented, and the interaction between the lime, silica and alumina to form silicates and silico-aluminates, which presumably was the chemical action causing the formation of the strong mortar, could proceed without interference.

In conditions applying in the field, however, the slaked lime would be subjected to carbonation by the carbon dioxide in the air at a rate which would probably be faster than that of the combination of the base and acids to form a hard mortar. It seemed also probable that the fine intermixing of lime and clay necessary to form a compact, homogeneous strong bed would not be secured. It might be that the tropical conditions under which Mr. Sykes' experiment had been conducted were a factor, but his suggestion was extremely interesting and might be worth investigating on a small scale under the cooler climatic conditions that applied in Great Britain.

Mr. Wooltorton dealt with an aspect of the Paper which had already been dealt with in the Author's reply to the Discussion (pp. 495 § *et seq.*), but he would reiterate that his Paper did not deal with the subject of soil-mechanics and that what was stated with regard to the material was intended only to be but a brief statement regarding its structure and characteristics, and as such was generally correct. The Author suggested that Mr. Wooltorton, in the penultimate paragraph of his observations, somewhat improperly took part of a statement which, shorn of its context, was not strictly true. It was, however, made quite clear in the Paper that the drying-out referred only to the surface and not to the whole mass of the clay. Methods were also suggested for rendering the surface stable. The Author noted with interest the last paragraph of Mr. Wooltorton's observations, and he hoped that the Building Research Station would supply the deficiency.

He was particularly gratified with the interest his Paper had created in the subject, and one of his objects would have been served if it resulted in more research work being undertaken in Great Britain in connexion with soil-mechanics, and also in the direction of assembling all the available knowledge of the subject for the guidance of the Civil Engineer.

§ *Ibid.*

Paper No. 5083.

"The Reconstruction of the Mocoretá and Timboy Bridges, Argentine North-Eastern Railway." †

By Geoffrey Charles Blofield, B.Sc. (Eng.), Assoc. M. Inst. C.E.

Correspondence.

Mr. H. F. Moloney, as Bridge Engineer on the staff of the Chief Engineer of the Argentine North-Eastern Railway, had prepared the plans of both bridges, and had been responsible for the general supervision of their construction.

An outstanding feature of the construction of the Mocoretá bridge was that the expenditure had been kept down to a remarkably low figure, notwithstanding the unexpected difficulties which had developed during the sinking of the cast-iron screw piles for the pier-foundations. The decision to use those piles had not been made on account of any anticipated advantage from using that form of construction, but had been based on the economies that would result, since the piles themselves as well as their caps and screws, and also the requisite plant for screwing, were all in stock and available; further, when the decision to use those piles had been reached, after a thorough preliminary examination of the ground by bore-holes and trial-pits, the existence of the large stones which subsequently caused trouble had not been revealed. In spite of the large cost of the salving operations and the necessity for re-pitching the piles and screwing them again, the piers as built were considerably less expensive than any alternative form of construction which would be suitable for the conditions of the site. The design of the abutments had only been settled after careful study of the relative merits and estimates made of the costs of both box and wing-wall types. The construction adopted had proved perfectly satisfactory, whilst the heavy stone pitching protected adequately the ends of the embankments against damage from scouring by the stream. During the course of the work, a minor addition had had to be made to the abutments, consisting of the reinforced-concrete struts which ran diagonally downwards from the small ballast-walls to the reinforced-concrete capping over the main pile-groups. It had been considered that the very small thrusts from the earth behind the ballast-walls would be sufficiently resisted by the transverse stiffness of the piles supporting them,

† Journal Inst. C.E., vol. 8 (1937–38), p. 527 (April 1938).

but, on placing the filling behind the walls, a slight movement had taken place towards the river, and it became apparent that additional resistance would be needed; the struts were designed and placed as being the best means to achieve that, transferring, as they did, the lateral stresses to the main bases which were amply strong to carry them. In the case of the Billingham Branch bridge [1], owing to the extremely soft nature of the ground encountered, somewhat similar strutting had been found to be required during the course of the construction. In the present case the ground had not been unduly soft, but the light ballast-walls, supported on long reinforced-concrete piles, had proved insufficiently rigid to resist the earth-pressure. After constructing the struts no further movement had taken place, and they had been covered by the stone pitching protecting the embankment.

The cross bracings and capping girders for the piers were prepared at the site, and certain work was carried out by the construction-forces which would be normally done in workshops. In that way, girders from stock were utilized after being cut to length and having stiffeners fitted in their webs as required. For the Timboy bridge, the spans were modified at the site. In both cases appreciable economies had been realized by executing the works at the site instead of handing over the steelwork to the company's mechanical workshops, which could not readily deal with them as they were not equipped for handling structural steelwork.

Since the completion of the bridge, a very slight lateral movement of the column-groups had taken place. That was checked at regular intervals and the extent of the movement was carefully recorded. It was thought that the small displacement was due to the columns taking up their definite set on the foundation stratum, and that, once that had taken place, conditions would become stable and all movement would cease.

The changing of the spans of the Timboy bridge had presented a difficult problem; when first examined, it had appeared that it would be necessary to build a temporary bridge with approach-deviations for the railway-track. The expenditure on those works would have been considerable, and accordingly alternatives which would obviate it had been examined. Finally, the scheme described by the Author had been adopted and had been carried out without a hitch. In working out its details the Author had displayed no little ingenuity, and the successful execution of the various and complicated stages had been mainly due to his close and careful supervision of every part of the work.

The method of supporting the old spans from the masonry piers prior to their removal, to enable their ends to be cut off, by taking advantage of the central arched openings in the pier masonry for some of the temporary beams, was novel on the railway, and gave excellent results. An essential

[1] W. P. Haldane and G. Roberts, "Billingham Branch Bridge." Minutes of Proceedings Inst. C.E., vol. 240 (1934–35, Part 2), p. 537.

part of the erection-scheme was the transportation of the 18-ton main girders from the working site to their final positions. The carrying of a pair of girders at a time on the specially-prepared railway wagons, which had to be moved very slowly, was accomplished with great care. That method of carrying the girders turned out to be a good solution of the problem of how to move heavy pieces of steelwork of unwieldly size, which were far beyond the capacity of the available cranes when travelling.

The Author, in reply, pointed out that the low cost of the works mentioned by Mr. Molony was undoubtedly due to the fact that they were carried out by the railway company's forces under competent supervision instead of by employing contractors, and would seem to indicate the advantage of carrying out works of a special nature in that way. The cost of sinking the cylinders, in spite of all difficulties, worked out at about half the price paid to contractors for work done in the locality during the construction of new lines some years earlier, although conditions had then been much easier and the quantities much larger.

The movement of the ballast-walls had been noticed as soon as the filling had been built up behind them, before any loads were superimposed, showing how little was the lateral resistance of the piles. After removing the earth-fill they were easily pushed back by jacking.

The bridge over the Mocoretá river had recently withstood successfully an abnormally high flood, during which the water flowed over and damaged the embankments but did no damage to the bridge.

Transporting the 18-ton girders for the Timboy bridge, after riveting up, had proved to be a simple matter by the method employed, which could no doubt be utilized for much larger and heavier pieces wherever sufficient clearance was available.

Paper No. 5130.

"Rapid Staining in Granites used in Civil Engineering Work." †

By Bernard Howard Knight, D.Sc., Ph.D., M. Inst. C.E., and Rena Gertrude Knight, M.A., M.Sc.

Correspondence.

Mr. Duncan Kennedy, of Beira, observed that the investigation made by the Authors was of considerable interest to those who might have to deal with Cornish granite, or granite with similar characteristics.

† Journal Inst. C.E., vol. 8 (1937–38), p. 545 (April 1938).

When objections were taken to the use of stone with initial iron-stain, or with a tendency to rapid staining after quarrying, those objections were generally for one or both of the following reasons:

(1) The possibility that the staining might indicate lower strength, or a liability to more rapid weathering.

(2) The unpleasing appearance.

In regard to the former the Authors stated on p. 550 § that "the staining is merely a surface change affecting the outer skin only of the rock, which detracts in no way from the structural strength of the stone." On the other hand, on p. 547 §, in reference to the fissures, either wholly or partially filled with strings of opaque iron oxide, that occurred in the type of granite liable to staining, it was stated that "In the more extreme cases, the fissures extend into the felspars, which are themselves frequently badly decomposed." That would appear to imply that there was some relationship between the iron-stains and the decomposition of the felspar. Mr. Kennedy had examined old walls in Devonshire, built with granite stones of colours ranging from pure grey to a dark rusty brown. Whilst the condition of the grey stone was as good as if it had been freshly quarried, the discoloured stones were in many cases badly weathered. It would be of interest to have the views of the Authors on that apparent relationship of colour and durability.

The question of appearance was bound to be a matter of individual taste, and each client was entitled to obtain the particular type of stone that he desired and specified. Mr. Kennedy had been told that architects sometimes asked the quarries to supply stone with a tendency to stain, on account of the warmer tone given to the finished work.

The trouble that had arisen at Chelsea bridge, referred to in the first paragraph of the Paper, had not been without fruit, in so far as it had inspired the investigation carried out by the Authors. That such trouble was avoidable, however, would appear to be the Authors' view, for on pp. 549 and 550 § they stated that "It should be stressed that in no case was the whole of a quarry found to yield 'honey-spotted' rock, and that careful selection of material will enable firms to supply a fresh grey granite which will retain its original colour."

The Authors, in reply, observed that the relationship between the decomposition of the felspar and the process of staining was that if the felspar were decomposed, but unstable biotite were absent, then staining did not take place. If, however, unstable biotite were present, then the staining was more marked in rocks with badly-decomposed felspar than in those which contained relatively fresh felspar.

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§ Page numbers so marked refer to the Paper. (Footnote (†) p. 530.)—Sec. Inst. C.E.

With regard to the condition of the grey stone in old walls, which was stated to be "as good as if it had been freshly quarried," the Authors would emphasize the fact that grey granites remained grey unless they contained unstable biotite. It would appear likely that the discoloured stone in walls which had weathered badly was brown throughout; that was to say, geologically weathered prior to being quarried. The grey stone which turned brown rapidly might occupy an intermediate position in regard to durability, although there was no definite evidence that it deteriorated in strength under the weather.

The Authors agreed that the warm colour of the stained stone might be preferable for some purposes. In the case of Chelsea bridge, grey granite was required by the engineers in charge of the work. There was no reason to believe that the stained stone was inferior in durability to the grey variety, but geologically weathered stone (brown to an appreciable depth) was considered to be less durable than stone which was grey when freshly quarried.

CORRESPONDENCE

ON PAPERS PUBLISHED IN

JUNE 1938 JOURNAL.

Papers Nos. 5172 and 5171.

"Constructional Work of the Fulham Power-Station."*

By John Findlay Hay, M. Inst. C.E.

and

"Fulham Base-Load Power-Station: Mechanical and Electrical Considerations."†

By William Clifford Parker, A.M.I.E.E., and Hubert Clarke, A.M.I.Mech.E.

Correspondence.

Dr. S. F. Barclay observed that the Authors' illuminating contribution to the study of fire-extinguishing by inert gas failed to direct attention to one point of outstanding importance to the power-station engineer, namely his obligation to have the gas-cylinders periodically emptied, examined, hydraulically re-tested and perhaps heat-treated. The following was the recommendation contained in the Fourth Report of the Gas Cylinders Research Committee ‡.

"Each cylinder should be submitted to the hydraulic test specified in Clause 13, at intervals of not greater than two years.

"Prior to each periodical test, the cylinder should be thoroughly cleaned and examined, externally and, as far as practicable, internally, for surface defects, corrosion, or foreign matter. Except where, in the case of very small cylinders, the opening of the neck is not sufficiently large, a small electric lamp inserted into the cylinder is useful for the purpose of internal examination. Where internal rust or foreign matter is observed, the cylinder should be heated to a temperature not exceeding 300 deg. C., and again cleaned and examined."

* Journal Inst. C.E., vol. 9 (1937–38), p. 3 (June, 1938).

† *Ibid.*, p. 17.

‡ H.M. Stationery Office, 1928.

At the present time there was legal compulsion to re-test only in respect of cylinders containing permanent gases and being conveyed by road, but consideration of the factors involved would leave few engineers satisfied to ignore the Research Committee's recommendations. The pressure within a cylinder containing liquefied carbon dioxide varied considerably even within the somewhat narrow temperature-variations to be expected in a power-station, so that the cylinder-walls were carrying a live and not a dead load. (The Report referred to contained full data on that point.) Further, dry carbon dioxide was without material effect on steel, but corrosion could become active if traces of moisture were present. The safety bursting disc with which each cylinder had to be fitted could take care of any undue pressure caused by abnormal ambient temperature, but could not safeguard against the effects of fatigue or of corrosion of the highly-stressed walls of the cylinder. The Authors described their installation as comprising one hundred and sixty-two cylinders and containing 12,960 lb. of gas: emptying, examining, and re-testing those cylinders every 2 years thus became a task of considerable magnitude.

Another feature of fire-extinguishing by carbon dioxide demanding the consideration of power-station officials was the effect of the liberated gas on the personnel. It had long been assumed that CO_2 was inert to human beings, and was undesirable in strong concentrations only in so far as it diluted the oxygen-content of the atmosphere. Recent medical research had shown, however, that CO_2 played a vital part in controlling the rate of breathing, the pressure of CO_2 in the alveoli of the lungs constituting the breathing "governor." The inhalation of the CO_2-concentration necessary for fire-extinguishing could result in a CO_2-pressure within the alveoli so much greater than the normal pressure that complete disorganization of the "governor" could result. It was for that reason that a person entering a place filled with CO_2 could fall dead almost immediately [1].

He desired to comment briefly on the statement advanced by the Authors to the effect that, although they recognized the rapid flame-extinguishment resulting from the application of the "Mulsifyre" system, nevertheless they felt that it was not due to emulsion-formation but rather to cooling. It was true that oil fires could be extinguished by cooling alone, but that could be demonstrated only under selected conditions of test, and could be proved by more searching tests to be unreliable. Under conditions such as could occur in actual practice, far from the water carrying out its intended function of extinguishing by cooling, the result could be highly-intensified burning. Simply by exposing any oil to the "Mulsifyre" discharge it could be demonstrated that an emulsion was formed; that the emulsion-formation and not the cooling was the cause of extinguishing could be proved in several ways—for example,

[1] Dr. J. Rambousek, "Industrial Poisoning from Fumes, Gas and Poisons of Manufacturing Processes." (Translated by T. M. Legge.) London, 1913.

by using a spirit with a flash-point well below the temperature of the water.

The importance of the difference between applying water in such a manner that an emulsion resulted and applying it in such a manner as to endeavour to extinguish the flames by cooling or by smothering following steam-formation could not be over-emphasized. What could those fighting an oil fire ask for more urgently than a method of converting the oil into something which could not burn? Such was the effect realized when a "Mulsifyre" installation came into operation: the burning surface of the oil was almost instantly converted into a non-combustible emulsion, and if the discharge of the water were continued the emulsion would extend down to a depth of inches below the oil-surface.

The Authors suggested that diffuser-nozzles attached to the ordinary firemen's hose could safely be relied upon to take the place of a fixed "Mulsifyre" installation. It should be pointed out, however, that the capabilities of the spray branch-pipe when tested under pre-selected and comfortable conditions on the demonstration-ground could give little indication of their limitations under actual fire conditions. The Chief Officer of the Enfield Fire Brigade, Mr. A. H. Johnstone, had had probably more experience than any other man in the country with oil fires of electrical apparatus. Although the Brigade was equipped with every up-to-date appliance, including hoses with diffuser-nozzles, Mr. Johnstone had expressed the following opinion [1]:—

> "From evidence gained during the outbreak, combined with previous knowledge of such risks, I cannot help but reiterate the view that oil-insulated high-tension electrical equipment demands a measure of fire protection on the fixed installation principle."

An oil fire within a building had to be actually experienced to realize the marked limitation of any hand appliance: dense smoke could obscure the target and the surging flames, perhaps carried many yards from the burning oil, could prevent the necessary approach.

The fire hazard of the modern power-station was too recent for reliable statistics to be available, but much could be learnt from a study of the fire records of analogous risks. The firemen's hose-pipe could dispose of any demonstration fire much more speedily than was possible with the automatic sprinkler, so that, applying the Authors' arguments, it might be said that the hose-pipe could reliably take the place of the automatic sprinkler. The result, however, of 50 years of actual fire experiences had led insurance companies to offer inducements to instal automatic sprinklers in the form of a rebate of at least 50 per cent. of the insurance premium, rising with certain risks to nearly 80 per cent. On the other hand, they

[1] "The Fire in Brimsdown's 33,000-volt Switchgear." *Fire*, November 1935, p. 127.

would not offer a rebate of more than 10 per cent. for the most complete installation of hose-pipes, fire-engines, and trained fire-brigades.

If there should be an oil fire where "Mulsifyre" equipment was installed, then, either automatically or by manual operation of the installation, the whole of the burning oil within the affected area would be instantly turned into a non-combustible substance. With hand appliances such as the diffuser-nozzle, there could only be a piecemeal attack of the conflagration: there would be no certainty of the flames being extinguished even in the accessible areas, and articles of equipment and structural parts would provide spaces behind which the flames could not be attacked. Further, the effectiveness of water discharged from the diffuser-nozzle was much diminished by the fact that it had often to be directed obliquely over the oil-surface, whereas with the fixed installation the water was beating down directly on to its target with full emulsifying power.

The Authors had done good service in pointing out that there was a real fire risk in the modern power-station. He would ask them to go further and to say that whatever precautions might be taken against that risk ought to be such that full reliance could be placed on them. To put in equipment which might or might not succeed against the menace of fire could hardly be seriously contemplated when the cost of a really reliable fire-fighting installation was itself all but negligible in relation to the other precautions taken in power-stations to ensure continuity of supply.

Messrs. J. L. Pearson, G. Nonhebel and **P. H. N. Ulander** observed that naturally the Authors had been precluded, owing to limitations of space, from dealing in considerable detail with the flue-gas scrubbing plant. In order to prevent possible misunderstanding, the following points might be mentioned.

The Howden–I.C.I. system, as installed at Fulham, was a non-effluent system. If desired, however, it could have been installed as an effluent system giving a discharge to the river conforming to the requirements of the Port of London Authority.

In the system, whilst calcium-sulphate supersaturation, seeding crystals and delay-times were important, as described in the Paper, there were other essential and characteristic features. Firstly, calcium-sulphite supersaturation in the scrubbing unit itself, when using coals of reasonably low sulphur-content, was prevented by oxidation or by utilizing a predetermined location for the alkali-addition so as to use to full advantage the changing solubility of the sulphite with change in the *p*H-value of the circulating liquor. With high-sulphur coals, the liquor-rates and the dimensions of the scrubbing unit and of the delay-tank could be dependent upon the degree of supersaturation of the sulphite to be permitted at the bottom of the scrubbing elements. The importance of the matter lay in the fact that sulphite scale on the scrubbing elements was far more difficult to remove than sulphate scale, the latter being amenable to a simple

treatment using soda-ash so that it could be removed (if it should form owing to some abnormality in running) without interfering seriously with power-station running. Secondly, the primary scrubbing elements in the scrubber were of fundamental importance as determining the degree of supersaturation permitted (for some supersaturation was inevitable), and as functioning as scrubbing surfaces where supersaturation was a maximum. Thirdly, the design involved proportioning the liquor-rate so that a permitted degree of supersaturation could not be exceeded, and choosing the dimensions of the delay-tank so that settling of the suspended solids was avoided.

The silting to which the Authors referred was certainly due to bad liquor-distribution, the gas-distribution being satisfactory. To solve the problem of liquor-distribution without experience from a full-sized plant was practically impossible. As mentioned by the Authors, the experiments to improve the liquor-distribution showed great promise, and their success would have a far-reaching effect on the reliability and maintenance cost of the plant.

Reference had been made to the corrosion of the brass interlacing strips of the grid packing. That construction of the grids was highly satisfactory for packing the grids into the cells; it had been used both for the pilot plant and at Swansea. Both those plants had been installed in conjunction with pulverized-fuel-fired boilers. It was safe to assume that the difficulties with the brass strips at Fulham were due to the combustion-conditions obtaining. With the retort-type stokers the fuel-bed was bound at times to be patchy. A blast of air passing through a temporary hole in the fuel-bed would give high local temperatures resulting in the formation of nitrogen oxides, which, if not counteracted, tended to cause excessive oxidation of the sulphite, denuding the liquor of reducing constituents and thus leading to corrosion.

As mentioned by the Authors, the work carried out by Imperial Chemical Industries in finding a suitable inhibitor had been partly successful, but, nevertheless, any new grids to be installed would be of a different construction, without brass interlacers.

Mr. Hay observed that the Correspondence did not call for any reply on his part.

Messrs. Parker and Clarke, in reply, observed that Dr. S. F. Barclay referred to the problem of maintenance of carbon-dioxide cylinders in the light of the Fourth Report of the Gas Cylinders Research Committee, published in 1928. That report, in their opinion, was not directly related to the problem existing in the case of static installations, such as carbon-dioxide equipment in power-stations, although the basic consideration of safety of vessels under pressure had to be borne in mind in the design of any such installations.

In the case of the carbon-dioxide equipment for fire-fighting that was under review, the working pressure of the cylinder was 750 lb. per square

inch and the cylinders themselves were tested at a pressure of 3,375 lb. per square inch. In that connexion it was interesting to note how that test pressure was determined.

Cylinders for use in the tropics were only allowed to be filled to 66 per cent. of their capacity, and cylinders in use in Great Britain to 75 per cent. of their capacity. With temperatures of 65° C. in the tropics and 45° C. in Great Britain the resultant maximum pressure developed at those temperatures and with the percentage fillings indicated was 1,800 lb. per square inch, and it was on the basis of that pressure that the test-pressure of 3,375 lb. per square inch was fixed. In the case under discussion, although the cylinders were in use in Great Britain they were only filled to the limit allowed for tropical countries, namely 66 per cent. of the total capacity. Further, they were housed below ground, so that the maximum temperature that could be expected, to which the cylinders would rise, was 25° C. It would, therefore, be seen that the maximum pressure which could be expected under those conditions was 925 lb. per square inch, thus giving a very wide margin over the test-pressure of 3,375 lb. per square inch.

Further, it had to be borne in mind that those cylinders were not being frequently discharged and filled. The gas was remaining static within the cylinder, and whilst there was some small degree of moisture in the carbon dioxide, which could not be removed by the manufacturers, the percentage was so small that the rate of corrosion was equally small; that, when coupled with the factor of safety provided did, in their submission, certainly remove the necessity of testing every 2 years.

With regard to the question of the effect of carbon dioxide on human life, that was a matter on which they had carried out investigations and actual practical tests. Their conclusions were that personnel should not enter into a carbon-dioxide laden atmosphere where the concentration was over 6 per cent., unless wearing some form of oxygen gas-mask. Since the concentration of a carbon-dioxide equipment, when the carbon dioxide was first released, was of the order of 50 per cent., it was obvious that with the use of carbon dioxide a prescribed routine had to be in force, whereby only trained persons wearing suitable gas-masks should enter a chamber containing carbon dioxide, and not until those people had given the "all clear" should ordinary personnel be allowed into that building. In their opinion, it was in no way an argument against the use of carbon dioxide as far as the actual building was concerned. However, carbon dioxide being heavier than air, it was essential that its users should realize the percolating powers of that gas to other chambers or buildings situated at low levels, and they should take all necessary precautions against any effects of that result.

Dr. Barclay, in referring to the "Mulsifyre" system, doubted Messrs. Parker's and Clarke's contention that there was a possibility that the fire was actually put out by the cooling effect of the water in the first place

rather than by emulsion-formation. They agreed with Dr. Barclay's statement that exposing oil to a "Mulsifyre" discharge created an emulsion, and that that emulsion could be formed to a considerable depth, but they still contended that with a fierce fire doubt was bound still to exist, and experiments which they had seen carried out definitely showed that with a diffused spray of water fires could be extinguished quite readily merely by cooling.

Dr. Barclay had also apparently misread their remarks in connexion with the displacement of "Mulsifyre" installations by hose-pipes with diffuser-nozzles. A reading of the Paper would show that they stated firstly, that they were carrying out further tests in connexion with diffuser nozzles and, secondly, that "if the results of the tests are as anticipated, it would seem to indicate that the provision of full automatic fire-protection, which is very costly, can be eliminated in many places and reserved only for vital key-points, where instantaneous action is necessary and where there is the possibility of operators not being in constant attendance." That statement agreed that fixed automatic fire-fighting equipment was essential in key-positions, and where constant attendance was not available, but implied that there were numerous other places which were not key-positions and where constant attendance was available, where the diffuser nozzle attached to an ordinary hosepipe would serve equally well at a much lower cost.

Finally, the methods described were in use on the Continent to a considerable extent, and had proved highly efficient.

Messrs. J. L. Pearson, G. Nonhebel and P. H. N. Ulander made reference to points in the design of the gas-washing plant, with which Messrs. Parker and Clarke were in general agreement. There were, however, several points mentioned which were, in their opinion, subject to doubt.

The first was in connexion with the question of corrosion. They did not feel that the question of corrosion could be quite so easily disposed of, as suggested by Messrs. Pearson, Nonhebel and Ulander, and in fact, so much so that whatever the type of stoking employed they could not from their experience recommend the introduction of brass interlacing strips for grid packing in the future.

That retort-type stokers were prone to uneven fire-beds and to have holes was, in the Messrs. Parker's and Clarke's opinion, very much subject to doubt. A certain unevenness of the fire-bed was bound to occur on occasions, but in general their experience of that particular type of stoker was that the fire-bed under normal operation represented a more or less uniform resistance to air-flow.

They believed that the question of oxidation in a gas-washer was controlled by numerous other points, and that had been demonstrated in the plant in question by the inability to control oxidation to within any real limits so far. Oxidation and corrosion were definitely bound together, and as far as the development of the gas-washing plant had proceeded,

they were of the opinion that the process had to continue to be run so as to produce a certain proportion of calcium sulphite in the system, purely from a point of view of economics.

On the question of inhibitors being used to prevent corrosion, whilst that had been proving successful to a certain extent up to date, they were of the opinion that the final operation of the plant should be such that inhibitors should be unnecessary, and the elimination of brass interlacers was a step in that direction.

Messrs. Pearson, Nonhebel, and Ulander appeared to feel that Messrs. Parker and Clarke were putting some slight condemnation on the plant due to operating difficulties which had been experienced. Messrs. Parker and Clarke would like to hasten to contradict any such impression, and to say quite clearly that the plant was new, involving numerous novel features and that it was only to be expected that difficulties would occur. However as stated in the Paper the efficiency of the plant in removing sulphur oxides from the flue-gases could not be disputed, as it was working with an efficiency of approximately 99 per cent. The main difficulties being experienced, which would be overcome, were high maintenance-costs and the limited running hours that the plant would remain in operation.

Paper No. 5154.

"The Reconstruction of Main Road Bridges, Calcutta." †

By Malcolm Ramsay Atkins, C.B.E., B.Sc.(Eng.), and Douglas Henry Remfry, B.Eng., MM. Inst. C.E.

Correspondence.

Mr. W. J. Doak, of Brisbane, observed that the Authors had shown remarkable courage in the adoption of arched bridges on yielding foundations. The elaborate precautions taken to prevent horizontal movements suggested that costs had probably been high, but, apart from that, settlements of 5 inches were bound to have produced some undesirable results in the structures.

Regarding the bearing power of piles, Mr. Doak had frequently endeavoured to reconcile the bearing power, computed more or less in the

† Journal Inst. C.E., vol. 9 (1937–38), p. 95 (June 1938).

manner given on pp. 106–107 §, with actual test-loads. The remarkable feature was that the latter had always been much the greater. That was still more remarkable when it was recalled that the frictional resistances to concrete caissons measured at the Zambezi bridge were less than Rankine's theory would indicate.

The figure of 110 lb. per cubic foot for w, the weight of soil, was very much suspect if the piles were in wet ground; the weight of soil would then be reduced by buoyancy to, say, 50 lb., which halved the figures both for bearing and frictional resistance. The source of resistance of driven piles was still an unsolved mystery.

In regard to the bowstring designs (which were actually tied arches), the method employed for computing moments in the arch-ribs would be of considerable interest.

Mr. J. P. Porter desired to confine his remarks to those parts of the Paper which referred to the supporting value of piled foundations, and to the calculations relating to the foundations of Beliaghatta bridge. He noted that the formulas for the supporting values of piles given on pp. 106 and 107 § were identical in form with those given in his own article on "The Supporting Value of Piled and other Deep Foundations *." Beliaghatta bridge had been constructed before the publication of that article, but the Paper had not been published until 1938. He would be interested to learn whether the Authors had evolved those formulas independently or whether they had been derived from his article.

It appeared to be desirable to state the derivation of his formula, which was intended to give the ultimate supporting value of any prismoidal deep foundation, whether driven as a pile, sunk as a cylinder or monolith, built up in an excavation as a pier, or formed as a pile bored in situ. The general formula was

$$U + P = \sqrt{l}A_b b + fp\frac{l^2}{2}$$

where U denoted the ultimate supporting value of the foundation-unit,
- P ,, ,, weight of the foundation-unit,
- l ,, ,, buried length of the unit,
- A_b ,, ,, effective bearing area (= 4 times the sectional area, in the case of a driven pile),
- b ,, ,, bearing-factor,
- f ,, ,, friction-factor,
- p ,, ,, perimeter of the foundation-member.

The value of b, the bearing-factor, was assumed to be $w\left(\frac{1+\sin\beta}{1-\sin\beta}\right)^2$,

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

* *Concrete and Constructional Engineering*, vol. 31 (1936), p. 319.

where β denoted the apparent angle of internal friction of the subsoil at the foundation-plane and w the density of the subsoil. The values of b suggested in his article had been derived from a detailed study of all data within his knowledge relating to the settlement of deep foundations and the test loading of piles.

The value of f, the friction-factor, was taken as $w \tan\phi\left(\dfrac{1-\sin\phi}{1+\sin\phi}\right)$ for values of ϕ not greater than 30 degrees, ϕ being the average angle of internal friction of the subsoil in contact with the sides of the foundation-member. Where the apparent value of ϕ exceeded 30 degrees, he had suggested provisional values of f ranging from 0·015 ton per square foot at 35 degrees to 0·025 ton per square foot at 45 degrees in the case of driven piles, and of 0·010 ton per square foot in the case of other deep foundations. The values of ϕ and f given in his article were based on a detailed consideration of a large number of skin-friction coefficients derived from the sinking of cylinders, caissons and monoliths and from the withdrawing of piles, as given in standard text-books and in the Proceedings of The Institution.

The values of the bearing-factor b and the friction-factor f which he suggested for use in his formula were as followed :—

Subsoil.	Bearing-factor b: tons per square foot.	Friction-factor f: tons per square foot.
Compact gravel	2·00 to 2·20	0·025 to 0·030
Gravel and sand, or very stiff clay	1·60 to 1·80	0·022
Firm sand, hard sand-clay or water-bearing gravel	1·20 to 1·40	0·020
Firm dry clay, chalk or moist sand	0·80 to 1·00	0·015 to 0·020
Soft clay or sand-clay, or wet sand	0·05 to 0·60	0·010 to 0·012
Peaty clay (" bungum ")	0·30	0·009
Soft wet clay, wet silty sand, peat or soft wet chalk	0·20	0·008
Mud or silt	0·15	0·007
Thin mud or silt	0·10	0·005

(NOTE.—Values of f for all foundations other than driven piles should be taken as not greater than 0·010 ton per square foot.)

The use of the term $\sqrt{l}$ in the formula for bearing resistance was based on practical observations of the test-loading of " Vibro " tubes as given by Mr. Alfred Hiley, M. Inst. C.E. The formula had been checked against all available data regarding the test-loading of driven piles, and in the majority of cases had been proved to be correct to within 20 per cent. The piles had been driven in strata of every type, and ranged from 8-inch timber piles carrying 10 tons ultimate load to 24-inch reinforced-concrete piles carrying 300 tons ultimate load.

It was to be noted that the factor of 4 applied to the bearing area of driven piles was equally applicable to piles cast in situ, provided that the

hole for the pile was formed by the dynamic driving of a tube or mandrel, but did not apply in the case of bored piles, where the subsoil was not pre-compressed.

With reference to Dr. Faber's remarks (p. 126 §), it should be noted that the formula allowed for a bearing resistance four times as great in the case of driven piles as in other cases. Thus the bearing resistance of a pile driven 40 feet and penetrating into compact ballast would be about 50 tons per square foot of pile section, whereas the bearing resistance of a non-driven member would be about 12 tons per square foot. Those figures were comparable with those cited by Dr. Faber.

Mr. Porter had recently had an opportunity, in the construction of piled foundations for large reinforced-concrete tanks, of testing the formula in practice against the ultimate-resistance values calculated from Mr. Hiley's formula. The results obtained had been such that he had been able to predict the ultimate resistance of piles of varying sizes and lengths driven into ballast and clay before the piles had been driven. By using those predicted values in Mr. Hiley's formula he had been able to estimate the probable driving sets for the piles at various levels. The sets so predicted had agreed very closely with the observed sets of the piles when driven. As the work was still in progress he was unable to give further particulars at present, but he hoped to have an opportunity of publishing them in due course.

It might be argued, in view of recent investigations into the theory of soil-mechanics, that it was irrational to use Rankine's formula for determining bearing- and friction-factors in cohesive materials. In his investigation, however, he had found that for all subsoils except rocks and semi-fluids it was possible to select values of β and ϕ which agreed reasonably with the results obtained from recorded data regarding foundations. There were indications, however, that the values used should be increased somewhat in the case of relatively deep foundations.

Mr. Remfry had apparently considered it advisable to check the supporting value of one complete foundation, firstly, as composed of a number of piles acting independently, and, secondly, as a pile-group in which the subsoil engaged by the piles was assumed to settle with them. Mr. Porter was doubtful whether the second assumption was justified in the instance under consideration, since (*a*) the average spacing of the piles in the main part of the foundations exceeded 5 feet between centres, (*b*) the silt had not been compacted more than about 4 per cent. by volume due to the pile-driving, and could hardly therefore be considered as being fully engaged by the piles, and (*c*) owing to the design of the raft, the surcharge of about 20 feet of soil at the rear, and the use of sheet-piling at the front, there was bound to be a considerable uplift under the raft.

The following remarks regarding the subsoil were to be noted. " . . .

§ *Ibid.*

the subsoil proved to be poor. . . . The soil below the raft appears to be fairly fine silt which extends for a considerable depth; its angle of internal friction appears to be about 19 degrees . . . the level of the subsoil water is kept constant by the near proximity of the canal. . . . Apparently the mean pressure over the base area under the original superimposed earth loads was about 2·55 tons per square foot. The site must have been well consolidated. . . ." In view of these remarks, he preferred to use a value for β of 25 degrees, giving a value for b of 0·30 ton per square foot and an average value for ϕ of about 15 degrees, giving a value for f of 0·007 ton per square foot.

He would first consider the foundation as being so supported by a number of independent piles that there was no actual pressure on the subsoil under the raft, and also that the uplift-effect could be neglected. Each pile would be assumed capable of carrying the full load calculated in accordance with his formula. Taking the assumptions given on p. 107 §, excepting as regards values of β and ϕ:—

Average bearing resistance per pile $= \sqrt{52} \times 5{\cdot}46 \times 0{\cdot}30$ tons $= 12$ tons.

Average frictional resistance per pile $= 0{\cdot}007 \times 4{\cdot}66 \times \dfrac{52^2-12^2}{2}$ tons

$= 42$ tons.

Therefore total supporting value of one hundred and thirty-seven piles $= 137 \times 54$ tons $= 7{,}400$ tons.

Therefore factor of safety on above assumptions $= \dfrac{7{,}400}{5{,}720} = 1{\cdot}3$.

The alternative method of treating the foundation as a single unit, in his view, was to assume that the average fluid pressure under the raft was that due to 20 feet of earth resting on saturated silt, that (in the worst case) the silt around the sides of the piles was, for the greater part of their length, fluid in nature and therefore virtually incapable of providing any support, and that the total bearing and frictional value of each pile in the apparently more compact material near the toe did not exceed 15 tons (the bearing value being 12 tons). The fluid uplift-pressure (due to 20 feet head of earth) under the raft was approximately 4,375 square feet $\times$ 1 ton per square foot $= 4{,}375$ tons. The probable minimum supporting value of one hundred and thirty-seven piles was about 15×137 tons $= 2{,}055$ tons.

Therefore, on the above assumptions, the factor of safety was $\dfrac{6{,}430}{5{,}720} = 1{\cdot}1$. It appeared, therefore, that the factor of safety slightly exceeded unity, whichever method of treatment was considered most applicable, but that it would have been advisable to have used longer piles.

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§ *Ibid.*

In accordance with the second treatment suggested, the majority of the load was transmitted to the subsoil at raft-level and exerted a downward pressure no greater than that due to the adjoining embankment. The load carried by the piles produced an additional pressure at toe-level of less than 0·4 ton per square foot. Mr. Remfry's contention that the foundation-plane should be capable of supporting a slightly increased pressure appeared to be justified from practical considerations, and Mr. Porter agreed that the method of treating pile-groups given in his own article was not, in that instance, applicable. It was, however, to be inferred from the care with which Mr. Remfry had examined the foundations in question that he entertained some doubts as to their suitability as foundations for a reinforced-concrete arch bridge. Those doubts were, in view of his calculations, fully shared by Mr. Porter.

Mr. Atkins, in reply, wished to assure Mr. Doak that no undesirable results had been produced on the structure of the Dum Dum bridge by the settlement of 5 inches which had taken place. The clearance at the hinged joints had in fact allowed for considerably more movement than had been observed. The lowering of the level of the roadway by 5 inches was of little importance, as the programme of operations allowed a period of 2 or 3 years to elapse between the completion of the bridge and the construction of the permanent approaches. With regard to Mr. Porter's remarks he confessed that some doubt had been entertained by both Mr. Remfry and himself regarding the suitability of the subsoil for resisting the thrust of a reinforced-concrete arch. He submitted, however, that the precautions taken had proved completely successful, and that the adoption of the design had been justified in practice, if not in theory. The approximate cost of the temporary bridges was: road-bridges, £2,000—£2,500 each; foot-bridges, £800 (Alipore) and £350 (Chitpore).

Mr. Remfry, in reply, observed that the maximum total settlement of the crown of the Dum Dum bridge was $2\frac{1}{2}$ inches, which might be the settlement due to the shrinkage of the concrete, plus 2 inches. In construction the centering for the ribs had been given a small excess rise which would take care of part of that settlement. The bowstring arches had been calculated as two hinged arches, the constraint due to the connexion of the tie beams having been disregarded.

In presenting the Paper the pile formula used was the one suggested by Mr. Porter, as Mr. Remfry considered Mr. Porter's views to be very well thought out and clearly presented. They did not, however, satisfactorily explain the supporting power of a pile-group under the particular conditions of the site. In the original design work it had been assumed that a raft on Calcutta soil would safely carry $\frac{1}{2}$ ton per square foot loading above the load carried at that level normally, provided that the raft was kept fairly high within the upper hard crust of soil. As a raft to carry the full load at that assumed loading would have been too large, piles were relied upon to supply the extra supporting power. No satisfactory

theory seemed to exist to determine the carrying power of a raft plus a pile-group.

The normal depth of soil in the bank above the bottom of the raft was about 14 feet, producing a pressure of 0·70 ton per square foot. At that depth the bottom of the raft should carry 1·20 ton per square foot, or a little more than the 1·0 ton assumed by Mr. Porter if an earth surcharge of 20 feet were assumed at the back of the foundation. As a matter of fact the earth surcharge of the roadway behind varied from about 18 feet at the back of the abutment to about 28 feet near the front edge, or an average of 23 feet, which might have given an average uplift pressure of 1·15 ton per square foot. In some cases there was no sheet-piling along the front edge of the foundations; that was used only at Dum Dum bridge and at Beliaghatta bridge, so that it might not be safe to assume the full uplift. Assuming an uplift of 1·15 ton per square foot at Beliaghatta bridge, the support would be 4,410×1·15=5,072 tons, leaving the 137 piles to support about 4¾ tons each, or 648 tons, giving a total of 5,720 tons. There was no doubt that the piles used could offer very much more support than that.

In regard to the carrying power of the piles at Beliaghatta bridge, the transition between individual action and group-action was apparently bound to depend upon the relative bearing support and the frictional support. If the piles acted individually there was a concentrated bearing support around the point of each pile, but the soil between those bearing points had to be capable of carrying the frictional support transferred from the sides of the piles. In fact, the soil between the piles might be considered as columns transferring the frictional support to the general area of the sub-grade adjacent to the points of the piles; there had to be sufficient area for that to be the case. It was scarcely reasonable to suppose that the intensity of pressure carried on the sub-grade between the bearing areas would be quite as great as that on the bearing areas; it would be absurd to expect that it would be greater. The following assumption regarding the relative supporting power of an average pile was therefore made: support from bearing area of 5·46 square feet, at a pressure of 1·37 ton per square foot=7·5 tons, frictional support=51·4 tons, total 58·9 tons per pile; hence from 137 piles, the total supporting power was 8,110 tons.

If that assumption were correct, then for the whole area, at pile-point level:—bearing of 137 piles with 745 square feet of bearing area, at a pressure of 1·37 ton per square foot=1,030 tons; frictional support spread over 5,850−745=5,105 square feet, at a pressure of 1·39 ton per square foot=7,080 tons; total=8,110 tons. That meant that between the bearing areas at the pile-points the excess pressure transmitted to the sub-grade was about the same as at such a bearing, the average spacing of the piles being 6·5 feet.

The conditions would be worse if a strip 9 feet were taken, where the pressure from the arch ribs was greater and where the piles were closer. Under such conditions the pile-spacing was 5·7 feet and the supporting area per pile at the pile-points was

32·3 square feet; hence 5·5 square feet of bearing at a pressure of 1·37 ton per square foot=7·5 tons, and 26·8 square feet of frictional support, at a pressure of 1·92 ton per square foot=51·4 tons. The frictional support could not exceed the bearing support, and undoubtedly a group-action would develop if the assumption made were correct.

From Mr. Porter's calculation of the supporting power of an average pile (namely, bearing support=12 tons and frictional support=42 tons (total 54 tons)), 137 piles carried 7,400 tons. If the whole side were considered, then 137 piles, with 745 square feet of bearing at a pressure of 2·2 tons per square foot =1,644 tons; 137 piles, with 5,105 square feet of frictional support, at a pressure of 1·12 ton per square foot=5,756 tons; total=7,400 tons.

Mr. Remfry considered that, if Mr. Porter's figures regarding the relative supports offered by bearing and friction were the more correct (as he thought that they might be), then the piles would act individually. If, however, the relative proportions as estimated in the Paper were correct, the piles would act as a group. He would suggest that when the frictional support per square foot between the bearing areas at the points of the piles exceeded 75 per cent. of the bearing pressure per square foot, then group-action might be considered as about to start. It was uneconomical to allow group-action to start, and hence it was important to ascertain correctly the relative frictional support and bearing support. It would have been better to have used fewer but longer piles. The handling difficulties, limited space, etc., precluded, however, the use of longer pre-cast piles, whilst the length of driving tubes available made it difficult to use longer cast-in-situ piles. Mr. Remfry did not believe that group-action had actually developed in the foundations. Mr. Porter had pointed out that the consolidation was insufficient for group-action, and he estimated the consolidation as only about 4 per cent.; possibly, however, it was nearer to 5 per cent. throughout the greater part of the area below the ribs. A direct connexion should be traceable between the amount of consolidation and the commencement of group-action in any particular soil. In any case, the bottom of a group had to be capable of carrying a greater concentration of load than obtained at that level before the group was formed.

The conditions in Calcutta for arch bridges were not ideal, but the difficulties could be, and had been, overcome. At Dum Dum bridge the settlement had been unexpected, but it was due almost entirely to the disturbance of the soil below the foundations which occurred in removing the existing foundations of a previous bridge.

Paper No. 5173.

"The Work of the Paint Research Laboratory of the London, Midland and Scottish Railway Company." †

By Frank Fancutt, F.I.C., A.M.I. Chem. E.

Correspondence.

Mr. C. W. Clarke, of Bombay, observed that the Author's recommendation that painting should not be carried out when the temperature of the structure was below the dew-point appeared to be substantiated by experience in painting coaching stock on the Great Indian Peninsula Railway. On that railway there were two workshops dealing with the painting of coaching stock. All the main-line stock was painted at Matunga, a suburb of Bombay, whereas branch-line stock of the northern section of the railway was painted at Jhansi, situated in the United Provinces, over 500 miles from the nearest point on the coast. The climate of Bombay was humid, whilst that of Jhansi was most arid. The labour in Bombay was more highly paid and in general greatly superior to that available in Jhansi, where the labour recruited was usually the local *ryot* (agricultural worker). Paints to the standard specifications and tests of the Indian Stores Department were supplied to both workshops in drums. Both workshops worked to the same painting schedule, and it was noticeable that the paintwork on a coach coming out of Jhansi workshops was superior in finish and durability to that of one from Matunga. In his opinion the only explanation possible was that the temperature of the steel body-panels on the coaches in the paintshop at Matunga was very often below the dew-point. In neither workshop was air-conditioning or air-heating employed. Abrasion of the paint-surface due to the great amount of dust in India was most pronounced, and he thought that the Author's test for resistance to surface-abrasion could be developed into a most useful physical test to include in any specification for paint for body-panels of coaching stock. Before such a test could be standardized, it would be necessary to specify the grain of the carborundum powder used, and the dimensions of the aperture at the bottom of the vessel containing the carborundum powder, as variations in either would affect the wear of the paint-surface under test.

The nature of the surface-soil had to be considered in selecting a suitable paint. For example, the soil in Western and Southern India was generally acid, whereas that in Northern India was largely alkaline, and the dusts from those soils affected the life of the painted panels on rolling stock.

† Journal Inst. C.E., vol. 9 (1937–38), p. 140 (June 1938).

Had the Author considered the effect of temperature on a painted panel? The surface-temperature on body-panels of coaching stock in India might exceed 150° F. for many hours each day during the hot weather, and that continued baking of the paint-surface tended to produce a very brittle skin, which accelerated peeling.

Dr. J. Newton Friend was glad to note that British railway companies were giving serious consideration to the question of paint. For many years the paint-industry had suffered from the popular misconception that any one could paint provided he was given a good brush and good paint. It was now beginning to be realized, however, that such was not the case. Shockingly bad results might be obtained with a first-class paint, simply because it had been badly applied. Further, it was essential to break away from the idea that, because a paint was well made and was suitable for a particular type of work, it was therefore the best to use for a different job. The user would have to learn to select the best type of paint for the work in hand.

The paints used by railway companies were required for three main purposes, namely protection, decoration, and intelligence. The last-named comprised those required for lettering, signals, etc., the colour playing an important part. It would be obvious that fading or darkening might be a very serious fault in such a paint whereas it might be no disadvantage whatever in a paint required for purely protective purposes.

The Author very properly stressed the importance of carefully preparing surfaces prior to painting, particularly in the case of iron and steel. It would be an enormous advantage if manufacturers could supply their steelwork covered with the thinnest possible layer of an elastic mill-scale which would cling tenaciously to the underlying metal, like the well-known skin on cast iron, and thus help to preserve it from corrosion. Actually, however, it was the tendency of the mill-scale to crack and fall away which rendered it so dangerous to steel structures exposed to corroding agencies. Corrosion became localized at the break, and not only might serious pitting occur but rusting proceeded, perhaps quite unobserved, under the still-adherent scale, gradually loosening it until it fell away in patches carrying its coat of paint with it. The serious nature of the pitting induced in steel by the presence of scale when exposed to sea-air and sea-water was well brought out in the photographs published [1] by the Sea-Action Committee of The Institution. There could be no question that the removal of scale prior to painting was most desirable. The problem was thus entirely different from that presented when steel was to be embedded in Portland cement. Many engineers imagined that the cement prevented the corrosion of the embedded metal because it kept the moisture away. That, however, was not the whole story, for cement was

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[1] Fifteenth Report of the Committee on the Deterioration of Structures exposed to Sea Action. H.M. Stationery Office, 1935.

porous. The real effectiveness of the cement lay in its alkaline nature. As the coating of cement was relatively thick, the alkali was not readily leached away, and any leaching action could be retarded by covering the cement with bituminous material. Hence there was no need to de-scale steel destined to be surrounded by cement, and that admittedly expensive treatment might be safely omitted. As the Author pointed out, however, a paint film was exceedingly thin and, although chromated pigments were often used in priming paints because they tended to inhibit corrosion, their value was reduced, particularly in subaqueous work, by leaching.

It would have been thought that the removal of scale by stretching the metal would have had a deleterious effect upon the subsequent life of the metal, but the proof of the pudding was in the eating, and the Author had not observed any such effect. That was very reassuring, and was gratifying to Dr. Friend, who had, for many years, felt that the effects of stresses on the tendency to corrode had been unduly magnified [1].

Mr. N. N. Maas, of Shanghai, observed that the Author stated that after specifications were drawn up to cover the quality of separate ingredients and proportions were standardized, the authorized formula was put into operation in the works and watched to ensure that the composition was being adhered to. The Author did not, however, mention whether the specification restricted the manufacturer in any way as to the method and machinery adopted in producing the paint from the ingredients. In few other process trades could there be more variety in regard to the procedure and the machines used in achieving the finished article than in the paint-making trade. An investigation would probably reveal that no one manufacturer produced paint in the same way as, and by similar machines to, those of any other manufacturer.

It would probably be agreed that paint did not merely consist of a complex of drying oil, pigment, drier and thinner, but that those substances had to be mixed, refined, and brought together in a special way; A difference in the process by which those operations were brought about (reference to which might be found in a Paper [2] by Mr. Maas) would undoubtedly be reflected in the qualities of the final paint. If the proportions of the ingredients could be accurately tested after the paint had been manufactured it would almost certainly be found that paint produced from the original formula, but by dissimilar processes, would show variations from the initial proportions of its constituents as well as differences in its physical properties. Too little attention had been paid to the actual mixing, homogenizing and refining processes in paint-making, whereas a wealth of industry, care and research had been lavished on the ingredients and their properties. There was certain to be an optimum process for

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[1] "A Study of the Resistance of Over-Stressed Wrought Irons and Carbon Steels to Salt-Water Corrosion." Journal Iron and Steel Institute, vol. cxvii (1928), p. 639.

[2] "Paint-Making Machinery." Trans. Inst. Engineers and Shipbuilders in Scotland, vol. 78 (1935), p. 413.

each particular kind of paint; in consequence, the particular process to be employed should be definitely specified.

The Author did not mention whether a maturing period was included in the specification. Paint taken directly from the final refining process or machine might not exhibit the same qualities as the same paint allowed to mature, say, for 6 months.

If the Author would express his opinion on those matters it would be of great assistance to other large users of paint, who would gladly welcome the information which the Paper imparted.

Mr. H. J. Troughton observed that on p. 145 § the Author gave a list of properties of paint; under No. 8 "Colour" he discussed shortly the question of comparing colours, remarking, "Instruments are available for measuring colour in terms of the three primaries, but those which are efficient are too costly for use in routine testing." That might be the case, but Mr. Troughton did think that some method should be evolved, even if it were not yet available. The cost of testing would presumably depend partly on the accuracy required, and it would not be necessary to make a routine test of every purchase of paint.

There were many colours used which were fairly well standardized, and the makers' usual colours were near enough. When, however, paints were required for rolling stock, especially road passenger-coaches and 'buses, the matter was rather more important, and one of the greatest difficulties that he had encountered was in keeping to standard colours when once they had been settled. Usually that was originally done by visual inspection, since æsthetic considerations were the most important, and for them the eye had to be used. After having chosen the colours, paint might be purchased from several makers over a period of years; if the vehicles were examined after a year or so many differences would be found, partly, of course, due to fading or darkening, but also due to the facts that samples had not always been matched from the originals, and that the originals themselves had altered when used for matching purposes.

Shortly after the War, when reds were not so good as they were now, he had had to deal with red vehicles and had purchased a tintometer; it seemed possible to keep the colour within a few shades of the original without much difficulty, and with a piece of apparatus which, to his recollection, was not very expensive. Recently, however, he had taken over a fleet painted blue and yellow; even in vehicles delivered by the same builders during the last few months there were variations in colour, and when the fleet was lined up in the depot the differences were very apparent. It did seem to him that some reasonably-priced instrument could keep such variations within narrower limits. He now had access to a tintometer in a nearby Technical College, and he hoped that in a year

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 9 (1937–38), p. 140. (June 1938.)—SEC. INST. C.E.

or so the wide range of shades on the vehicles would be limited, but, more important still, the original approved colours should be capable of reproduction without much trouble at any time within fairly narrow limits.

The Author, in reply, observed that the points raised served to emphasize the extreme complexity of the subject, and suggested a wide scope for future investigations. It was important to appreciate, as Dr. Newton Friend pointed out, that a paint suitable for one situation might be quite unsatisfactory in another. The Author's aim had been to develop paints which gave satisfactory service over the widest possible range of conditions, but even so, the number of paints necessary to meet the diverse circumstances encountered within the L.M.S. Railway Company's activities was very considerable.

The greatest importance was attached to correct methods of manufacture, which should ensure that the manufacturer was properly equipped before contracts were placed, with the object of ensuring that properties such as satisfactory wetting of the pigment and the optimum particle size of distribution, were properly safeguarded. The stress laid by Mr. Maas on that aspect of the subject was well placed.

With regard to a maturing period, although reference to a particular period did not appear in the L.M.S. Railway Company's Specifications, steps were taken to ensure that materials when received were in a condition suitable for immediate use.

The Author did not wholly share Dr. Friend's view with regard to the need for de-scaling of steel to be encased in concrete, since stray electric currents might bring about that corrosion which it was so necessary to avoid; he tended to the view that it was better to de-scale and coat the steel with an insulating material, such as bitumen, before it was encased in concrete.

Some years ago, an experiment was carried out in order to discover what the effect was on painted surfaces of temperatures of up to 180° F., and the results showed that temperatures of the order mentioned were without appreciable effect upon the durability, but that the destruction of the paint-film was rapidly induced by light from the ultra-violet end of the spectrum. Mr. Clarke's details relating to his experience on the Great Indian Peninsula Railway were particularly interesting.

The advent of a cheap and efficient tintometer that would permit rapid comparison of colour and be generally suitable for routine work would be welcomed. Provided that the material conditions were carefully controlled, the visual examination at present used was satisfactory for most practical purposes.

Paper No. 5174.

"Southampton Docks Extension." †

By Malcolm Gordon John McHaffie, M. Inst. C.E.

Correspondence.

Mr. John Anderson observed that the Port of Southampton was to be congratulated on the extension of its already considerable facilities, both mercantile and industrial, by the simultaneous acquisition of valuable new land and of docking and berthing amenities of the highest class.

The layout advocated by the late Sir Frederick Palmer had been supplemented by structural and constructional recommendations aimed at reduction of the estimated unit cost of the works (apart from the saving effected in length of quay), and a few points relative to the quay-wall design, in connexion with those early considerations of the scheme, might be of interest. The problem of selecting a quay of suitable section for the tidal conditions and traffic-requirements was exceptional, providing as it did for a height of 64 feet above dredged level. Alternatives were, broadly speaking, limited to either solid monolith construction, or open quay of really stout construction.

In view of the adoption of the 45-foot monolith wall, as originally designed and actually constructed for a short length, it was interesting to note the conviction expressed by Mr. Wentworth-Sheilds on p. 223 § that "big quay-walls" of that type were "not really economical," and his reference to the handicap occasioned by the lack of a "rational theory of earth-pressures." Were the limitations of that type of wall to be attributed to the theory of its design rather than to inherent difficulties of construction? On investigating the required section for the Southampton quay-wall Mr. Anderson came to a similar conclusion, namely, that a wall of such height was penalized by the accepted theories. He found that the orthodox treatment, when applied to a monolith-wall of great height, produced some unsatisfactory conclusions and more or less obvious anomalies as soon as the limiting toe-stresses and passive resistance became critical factors of the design. Owing to the rapid increase of depth and width of wall needed to satisfy those requirements, a point was reached where the unit cost of solid quay section became uneconomical, and an engineer might reasonably be influenced thereby to modify his theory, or to abandon the type of construction.

There was little doubt that the assumptions of orthodox analysis erred

† Journal Inst. C.E., vol. 9 (1937–38), p. 184 (June 1938).
§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. Inst. C.E.

tremendously on the safe side, both in regard to the position of the centre of earth-pressure and its magnitude. From various analyses giving special consideration to those factors in the case of Southampton quay-wall, he found that a section from 15 to 20 per cent. lighter might have been considered adequate. The theory on which such analyses were made was based on the assumption of a virtual arch action developing in a vertical plane in the backing material as soon as passive resistance came into play. Although simple observations and experiments appeared to substantiate that theory (which might explain the continued safety of some walls of whose stability doubts might be cast by orthodox analysis), there was need for more definite knowledge and a sound basis of treatment. Mr. Wentworth-Sheilds had already presented an excellent approach on orthodox lines to the analysis of moderately large quay-walls[1], and it was encouraging to learn from him that Dr. Stradling was now at work on a theory of earth-pressure which might justify an economy of design not hitherto considered safe by responsible engineers. Dr. Stradling had recently emphasized the value of practical investigation in the field of everyday engineering experience, as compared with laboratory experiments on soil mechanics, and Mr. Anderson would suggest that problems affecting the design of quay-walls particularly seemed to call for large-scale test observations.

Reverting to the Paper, there were some points upon which further light might be sought. The Author, in describing the modified quay-wall, stated that it had been decided to compensate for the loss of stability incurred (through decreased sinking-depth), by removing the gravel behind and forming it to a natural slope of 1 in $1\frac{1}{2}$. It would be interesting to know what horizontal pressures had been allowed for, resulting in the retention of the original 45-foot monoliths. It appeared on casual observation that they were, in effect, merely piers planted in the slope for the support of vertical loads and subject to very little unbalanced horizontal thrust. In that connexion it would be useful to know whether the 2-inch tilt of the monoliths mentioned by the Author referred to the original section or to the revised section, and, if to the latter, whether the slope was grabbed out behind prior to the removal of the material in front. Were any observations taken to show whether the movement was due to tilt or to lateral displacement of the monoliths as a whole ?

With regard to the work as carried out, it would be helpful to have any information as to the behaviour of the slope behind the monoliths under the scouring action of tidal ebb and flow and of ships' propellers. Had the pitching of the slope been placed by hand above low water? How far did the slab-beam over the piles extend into the top of the slope? It appeared to be rather shallow to prevent some spewing under it, unless the backing material had been specially selected.

[1] "On the Stability of Deep-Water Quay-Walls". Minutes of Proceedings Inst. C.E., vol. ccxiii (1921–22, Part 1), p. 135.

It was good to learn from Mr. Szlumper's remarks on the modified quay-wall that the cost of additional work of grabbing-out the slope and constructing the 4-foot 6-inch slab, piling, etc., had shown a saving in cost compared with 23 feet of monolith-sinking. It was also interesting to note that the natural slope of the ground was found to be so steep as 1 in $1\frac{1}{2}$, because at the time of preliminary investigation it had been considered improbable that the ground would stand even at 1 in 2. That fact would certainly have favoured the construction of the type of quay-section recommended by the Consulting Engineers, at an estimated saving of £66 per foot run of wall, as compared with the solid 45-foot monolith construction. That section consisted of 20-foot-square monolith piers, arranged in rows of four at 60-foot centres, and driven to the depths which had since proved practicable in the actual work. Those piers were to support a massive deck of concrete and steel girders over natural ground-slopes of 1 in 2, capped behind the quay by a blockwork wall above L.W.O.S.T. for retaining the reclamation-material. The monoliths were to be connected together so as to form a triple portal-frame which, with suitably-arranged steel, would provide adequate support to such side-thrust as might have to be resisted from the material bearing upon the block-wall and rear monoliths. The principle of that method of frame-construction was now being used on a smaller scale at the Victoria dock of the Port of London, with advantages which would increase with the depth of berth. It would be seen that the monolith-work involved in the piers was about 33 per cent. less than in the modified quay-wall, the units being smaller and possibly more manageable. As an interesting possible alternative that scheme presented distinctive features which might appeal to engineers contemplating similar problems.

It was interesting to note Sir Henry Japp's suggestion that, at some future date, jetties might be built on to the existing straight quay, because it raised an aspect of the layout which might be overlooked. The five jetties which could have been constructed on the site available to provide equivalent accommodation would have precluded for navigational reasons the construction of the proposed future jetty (approximately 10,000 feet of quay). Further extension then could only be provided by exploiting an entirely new area, such as the Marchwood foreshore. If provision had been required for a smaller class of ship, say from 500 to 600 feet in length, it was probable that the advantages would have been in favour of the jetty layout for economic and navigational reasons, without sacrificing future developments.

Mr. E. J. Buckton observed that the Paper gave a most useful and concise description of the most important work of dock development in Great Britain in the post-war period. In 1926, Messrs. Rendel, Palmer & Tritton—of which firm the late Sir Frederick Palmer, Past-President Inst. C.E., had been senior partner—were asked to advise the Southern Railway on a proposed jetty scheme for extensions at Southampton docks,

and it might be helpful to give a brief statement of the views of the Consulting Engineers.

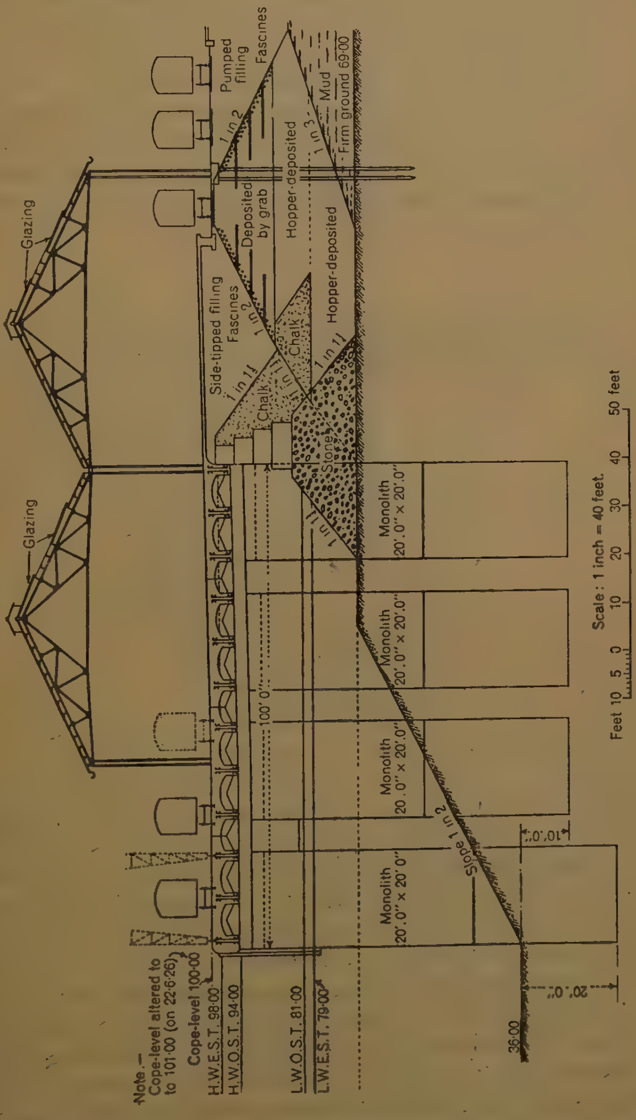
Only one scheme had been submitted by the Company to the Consulting Engineers, which was the "five-jetty" scheme mentioned in the Paper, carried to a very full stage of development. After close and friendly collaboration with Mr. Wentworth-Sheilds, who was at that time Docks Engineer for the Railway Company, and with the Author of the Paper, who was then the Chief Assistant, the Consulting Engineers had advised the abandonment of the jetty scheme and recommended a straight quay-wall scheme, giving their reasons for the recommendation and at the same time outlining and recommending a particular form of construction. In due course the Company accepted the layout for a straight-line quay, but did not accept the Consulting Engineers' recommendations as to the method of designing and constructing the quay. The Company decided instead to construct the quay of unusually large monoliths in a continuous row. The Consulting Engineers, who had been responsible for the continuous monolith quays for the Tilbury Docks extension which were proving quite satisfactory, had carefully considered the same method of construction for the quay at Southampton, but, as the quays at Tilbury were in an enclosed dock whereas at Southampton they were on an open tidal waterway, the depths in the latter case were much greater and the conditions became much less favourable for monolith work. After the most careful consideration as to constructional methods and cost, the Consulting Engineers had reported as followed:—

> "Our first proposal was the building of a quay wall on a continuous row of monoliths—a method of construction largely used where foundations are a difficulty—but it was found that the cost would be practically the same as for the reinforced-concrete method. Eventually it was decided to recommend the design now proposed, which consists of a series of monoliths sunk at right angles to the quay face, forming piers, between which the quay, and the inner half of the shed behind the quay, is carried on steel girders embedded in concrete.
>
> "The cost will be less than for a continuous quay wall on monoliths, and, as will be seen, less than the estimated cost of the reinforced-concrete quay proposals."

The elevation and cross section of the quay as recommended by the Consulting Engineers were given in *Figs. 23* and *24* (p. 558). It was true that the proposed construction as there illustrated included monoliths, but they were separate monoliths of modest dimensions, acting primarily as piers. At Tilbury the quay was formed by a continuous row of monoliths of large dimensions, designed to retain the filling behind, with its surcharge, the principal duty of the monoliths being to resist a horizontal thrust. In the design for Southampton as recommended by the Consulting Engineers

the principal duty of the monoliths was to carry a vertical load. The monoliths were in no way continuous; they were arranged in rows of four at 60-foot centres, so that the whole structure became an open-deck quay,

Fig. 23.



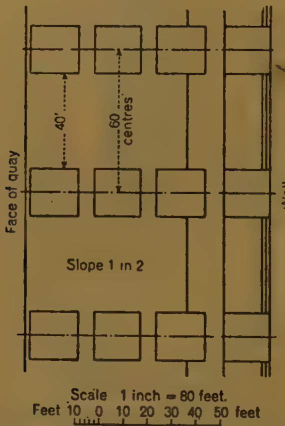
Cross Section of Quay-Walls and Sheds.

with a revetted slope of 1 in 2. During the discussions fear was expressed by the local engineers that the ground at the site was of such a nature that it could only be held by a continuous wall or a very flat revetment, and

it was possibly that view which led to the ultimate adoption of a continuous wall of large monoliths capable of withstanding the thrust of the quay, with its surcharge. That view was in no way shared by the Consulting Engineers. Difficulty was experienced in trying to sink a continuous row of large monoliths in the conditions existing at Southampton, and the design was modified to lessen the horizontal thrust on the monoliths by adopting a decked quay with a revetted slope of 1 in 1½.

Costs were not given in the Paper, but the cost of trying to sink the monoliths to a designed depth and, later, in abandoning that principle and adopting a scheme dependent upon the retention of a slope behind the monoliths, decked over in reinforced concrete, was bound to have been unfavourable; a better and cheaper structure would, in Mr. Buckton's opinion, have been obtained if an open-decked quay had been adopted in the first place.

Fig. 24.



HORIZONTAL SECTION SHOWING ARRANGEMENT OF MONOLITHS.

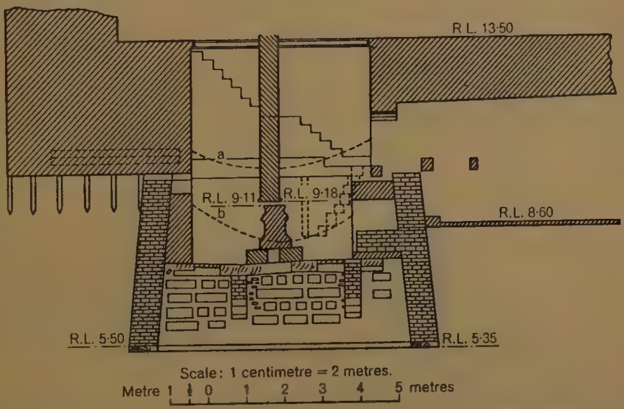
The relative merits and demerits of a straight-line quay, as compared with a series of jetties, were fully discussed during the investigations and were clearly stated in the Consulting Engineers' report. It was claimed that 7,500 feet of straight quay were equivalent to 10,000 feet of jetty quay. The estimated cost of the shorter straight-line quay was more than £1,000,000 less than the jetty scheme, and whereas the jetty scheme utilized the site to its full capacity the straight-line scheme as recommended and adopted had the valuable asset of permitting a further 150-per-cent. extension as, and when, required—an important factor, since suitable areas for port-development at Southampton were very limited.

There were many other matters of interest in the Paper, but Mr. Buckton had confined his comments to that phase which especially con-

cerned the late Sir Frederick Palmer and the Consulting Engineers, as it appeared desirable that certain ambiguities, perhaps inevitable in so concise a Paper, should be cleared up.

Mr. K. O. Ghaleb, of Cairo, observed that the method of construction of the dock described on pp. 198–204 § was practically the same as that adopted to permit examination and repair of the square well enclosing the famous column of the Roda island Nilometer. That monument for the gauge-recording of the river-levels had been built in the year 247 of the Mohammedan Era (A.D. 861); it was therefore the oldest Arab monument in Egypt, and was unique in its conception. The well was supposed to be cleaned each year of the silt that accumulated during the previous

Fig. 25.



SECTIONAL ELEVATION (FROM WEST TO EAST) OF LOWER HALF OF WELL AS FOUND IN 1937.

flood; that cleaning had never been properly done. The deposit was generally only cleared as shown by the dotted curve "a" (*Fig. 25*); during the French expedition, at the beginning of the nineteenth century, General Menous' engineers succeeded in clearing the well down to the dotted curve "b" (*Fig. 25*); further attempts made in 1887 and 1927 were less successful. From the information left by the French expedition, there was no evidence that any part of the structure extended below the level of the 1·20-metre-high marble base, and it was concluded that the bottom of the well-chamber was paved with flag-stones at that level. The column was badly handled during the nineteenth century, and during the latter half it started to sink gradually. It was difficult to know what to do: the bottom of the well had probably remained unseen from the time of its construction, and no ancient manuscript

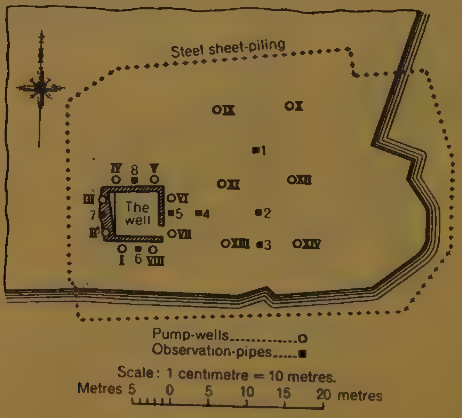
§ *Ibid.*

giving a reliable account was available (although the notes left by the French expedition were very useful). The last attempt, at the beginning of 1927, to unwater the cistern by pulsometers had to be abandoned for fear of the collapse of the whole structure; the column had to be propped up in a most unsightly fashion.

After a long and careful study of the question, it was agreed that the modern method of drying the ground was the most promising. It was stated in the conditions of tender that payment to the contractor for unwatering would only be made from the date of his lowering the water to such a level that work in the bottom of the well could be possible.

The eastern side of the southern extremity of the island (*Fig. 26*), on which is built the Nilometer, was enclosed during 1937 by steel sheet-piling between R.L. 17 and R.L. −3 metres on the land side, and R.L. 17 and

Fig. 26.



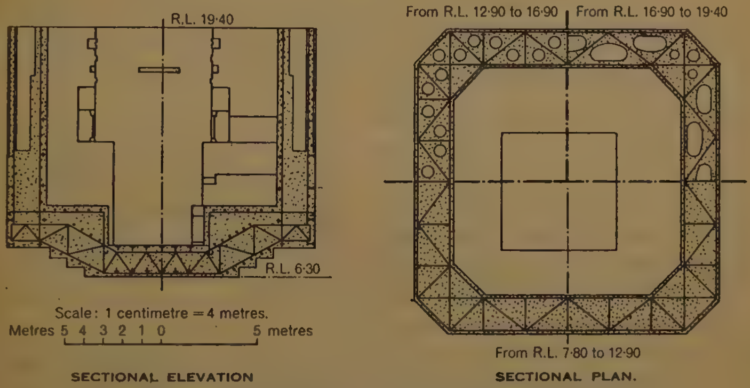
SITE-PLAN.

R.L. −5 in the Nile. The water-level of the Nile during the period of the operation of the pumps was R.L. 15·8; the average level during flood was R.L. 20·0. Trial borings to indicate the line along which the sheet-piles were to be driven disclosed that this extremity of the island, to a depth of about 10 metres, formed a huge mass of large and roughly-built calcareous stones with sandy clay underneath. Most of the stones utilized had been removed from the ancient monuments of Heliopolis. For lowering the water-pressure, fourteen holes were bored, two on each side of the Nilometer well and six on the eastern side (*Fig. 26*). At each hole a tube 0·40 metre in diameter was sunk to R.L. − 6, penetrating 22 metres into the water-bearing stratum, and into it was placed a 0·2-metre diameter pipe, the lower end of which for a length of 11 metres was perforated and wrapped with a fine copper mesh to form a filter. The annular space around the inner pipe was filled with clean fine gravel in the

vicinity of the filter, and with puddled clay above it, the placing of the fine gravel and puddled clay being done concurrently with the jacking-out of the larger tube. Each tube was equipped with an electrically-operated submersible pump 0·125 metre in diameter, discharging through mains into the Nile. The total quantity of water pumped amounted to 3¾ million cubic metres; the water was pure and clear. Pumping was begun at the end of February; by the end of March the mean water-level in the four observation-pipes around the well (*Fig. 26*) had been lowered from R.L. 15·8 to R.L. 7·5, and it was kept between R.L. 7·5 and R.L. 5·5 until the end of June, when pumping was stopped.

The unwatering proved the existence of an unknown brick well (*Fig. 25*) lined inside with calcareous stones similar to those used for the main structure: they were found detached from the brick well. The latter was round (whereas the main well resting on it was square), slightly tapered,

Figs. 27.



Reinforced Concrete Around and Below the Well.

and was built on a wooden curb; inside it, but at a higher level, was a smaller one not concentric with it, also built on a wooden curb and filled with calcareous stones. That smaller well carried the wooden platform on which was placed an old granite millstone, which in its turn supported a marble base 1·2 metre high which carried the column of the Nilometer. The lower part of the well was nothing but confusion: the column, base, and platform were found to be separated from each other and engulfed in 4 metres of silt. The monument might have collapsed at any moment. The Royal Botanic Gardens, Kew, kindly analysed nine specimens of wood found in good condition inside the well, though it had been there since A.D. 861. With the exception of the circular platform that supported the mill-stone, the wood found was in a spongy state, but after drying it shrank and became very hard; the cypress beams of the timber flooring of the well, found at R.L. 7·63, still kept their pleasant aroma. The iron nails, some of them over 30 centimetres in length, that

kept the woodwork together showed no trace of rust, whilst the lead and bronze props used for fixing the column had not deteriorated after having been buried for over 1,000 years.

The Nilometer, being an historic monument of primary importance, had to be maintained in its original condition; to save the well from collapsing it was decided to fix a "chemise" around it, and to place a solid floor underneath it by underpinning (*Figs. 27*). As the well was to remain dry in the future, the bottom of it would have to bear an upward pressure nearly equal to that of the first Aswan dam; the heavy floor shown in *Figs. 27* was therefore necessary.

The contract cost would amount to about £E35,000; the main items were :—

(i) Steel sheet-piling, 160 metres in length, weighing about 450 tons, at about £E7,000.
(ii) Unwatering, first 90 days at £E225 per 24 hours and afterwards reduced to £E24 per 24 hours, amounting to about £E20,000.
(iii) "Chemise" and flooring, 1,600 cubic metres of ordinary concrete and reinforced concrete, amounting to about £E6,000.

Mr. H. J. B. Harding observed that in the discussion on the Paper, Mr. W. T. Halcrow had asked several questions about ground-water lowering which showed that he was under a slight misapprehension as to what had actually been carried out. Perhaps Mr. Harding, as a member of the graving-dock contractors' staff, might be permitted to enlarge on the details which the Author had given.

The ground-water lowering method employed was not the well-point method, but was a deep bored-well system of ten wells, using submersible pumps, which were all electrically driven from a central switchboard. The term "well-point method" was usually adopted where a number of small-diameter tubes were jetted or driven into the ground to a depth of about 25 feet at very close intervals, and water sucked from them. That method could not be used to reach artesian water at over 100 feet below ground-level, which could only be reached by means of bored wells penetrating deeply into the artesian strata.

Mr. Halcrow had doubted whether the fine copper-mesh filter would have worked satisfactorily if the sand had been very fine. In actual fact, the sand at Southampton was particularly fine, interspersed with layers of clay. The filter-wells also tapped each layer of sand as they passed through it, as stated in the Paper.

The wells at Southampton were about 200 feet deep, the diameter at the bottom being 24 inches. The filter-tubes in the wells in which the submersible pumps worked were 14 inches in diameter to give room for the pump, but there was an extra refinement at Southampton in the case of the permanent wells. It was known that eight of the ten wells were to be permanent, and that the strata from which they were to extract the

water was lower than the actual working level of the pumps, as the water rose to the pumps under the artesian pressure. Advantage was taken of that to make the lower portion of the filter-tubes in the permanent wells only 6 inches in diameter, which gave more room than was normally the case for forming a gravel filter inside the 24-inch boring tubes.

Two different grades of filter-gravel were used, the finer material being outside, and the two filter-materials were kept separate during placing by a tube about 14 inches in diameter, which was withdrawn simultaneously with the boring tube, and ensured that the different grades of gravel were deposited correctly. The actual permanent filter-tubes were made of tinned copper, 6 inches in diameter, $\frac{1}{4}$ inch thick, with $\frac{3}{32}$-inch slots (fine enough to hold back the coarser filter-material), and not covered with fine mesh, as was the case with temporary wells, as it was possible to get a 9-inch thickness of filter-gravel, making very fine mesh unnecessary. In a properly-constructed water-lowering system, the gravel filter should be relied upon to keep back the sand, and the mesh on the filter-tubes was an extra precaution. The filter-gravel prevented the fine sand clogging the mesh on the tubes, as very little reached the tubes.

There should be no difficulty in obtaining a proper filtration without clogging, however fine the sand, as, if water could flow in the sand itself, it would flow into the wells, and if the gravel were graded correctly, that should be possible without sand being carried with the water. The fineness of the sand at Southampton was shown by the fact that the overflow in the permanent system was only about 250 gallons per minute from all eight wells, although the overflow-level was 50 feet below the artesian water-level.

From the above description, it would be seen that there was every reason to expect a long life from the filters. It was possible to clean them out or back-flush them if required, and there was still room inside the well for a smaller filter-tube if that should ever be necessary.

The difficulties that Mr. Halcrow said he had experienced in forming a filter in the bottom of a cylinder might have been due to a wrong grading of the filtering material, and to the fact that the water had to enter under the bottom of the cylinder, and not through the sides, as with a filter-well.

The hydraulic principles governing the flow of water into artesian wells in porous formations showed that the quantity of water entering depended chiefly on the thickness of the porous strata and on the "draw-down" in the well. The "drawdown" was the cause of the curve of depression assumed by the water-table, and brought about the difference in head which caused the water to flow into the well at different rates, according to the permeability of the ground. The water entered the well throughout the area in contact with the porous strata, even if, as at Southampton, several different beds were met with, separated by layers of clay.

In the case of a filter at the bottom of a cylinder, it was impossible to obtain a deeper draw-down than the bottom of the cylinder, and all the water had to flow into a single thin horizontal bed. The depth of the cylinder in relation to the formation-level would be important. In a bored filter-well, the water was tapped for the whole depth of the bed by the vertical filter, into which it could freely flow, and in which a very considerable draw-down could be created by the submersible pump. Further, it was not always understood that one of the chief features of a water-lowering scheme was the great effect produced in lowering the water-table by the use of a number of wells, as they "interfered" with each other, and their cones of depression intersected before reaching their individual levels. That could be better understood by referring to *Figs. 14* and *15* (pp. 202–3 §) of the Paper. The effect of one well on the water-table would be very slight beyond a very short distance, but ten wells working together and spaced round the site lowered the water-table to a remarkable extent, with very little more total quantity of water pumped.

The 2-inch sounding wells described by the Author were essential for the proper control of the system while working. They were sunk first as a guide to the strata, before sinking the expensive 24-inch-diameter wells. At Grimsby Fish dock the sounding wells revealed a thinning-out of the porous strata and saved the sinking of wells in an unproductive area.

The Author, in further reply to the Discussion, and in reply to the Correspondence, wished first to correct a mistake in his reply to the Discussion; on p. 236 §, line 14, the cost of the graving dock should have read "about £1¾ million."

The nature of the damage to the monoliths had been principally opening of the horizontal joints, in some cases to the extent of several inches, which indicated fracture of the vertical steel reinforcing bars at those places. The damage had not occurred in any particular zone, but had been confined to the blockwork above the reinforced-concrete shoes. No damage had been observed in the shoes themselves, which appeared to prove that they were sufficiently strong and stiff. The worst damage had occurred in the external transverse walls, namely, those adjacent to other monoliths.

The rate of sinking had varied considerably, according to whether it was being done in the gravel embankment or in the virgin strata beneath it, but taking a representative period when the work was in full swing, and disregarding the gradual working up to speed in the early stages and the gradual falling off in the later stages, the average rate had been about 60 feet per week.

No very definite figure could be given for the sinking effort, as several of the factors which had to be taken into account in assessing it were

§ *Ibid.*

variable. For instance, the flotation-value varied with the rise and fall of the tide and the amount of pumping it was considered advisable to do in the wells; again, the bearing value of the ground against the side of a tilted monolith and that of the ledge of firm greensand upon which the cutting edges rested during sinking, were not easily determinable. Having regard to all the circumstances and assuming certain values, it had been calculated that the sinking effort in the later stages of sinking was between 8 and 10 cwt. per foot super of external wall.

Regarding the limitations of the size of quay-walls, the large gravity retaining wall was not really economical because hitherto it had been impossible to ascertain with accuracy the pressures and resistances imposed upon it, and those were therefore often over-estimated; and also because the material of such a wall was subjected to stresses which were for the most part far below its safe stress.

Upon reference to p. 197 §, Mr. John Anderson would see that the Author stated that the loss of stability was compensated for by "removing the gravel behind and forming it to a slope of 1 in 1½ inch; not to a "natural slope," as stated by Mr. Anderson. It was true that had the material been removed to its natural slope the monoliths would theoretically have been subjected to little or no unbalanced horizontal thrust, but 1 in 1½ was not the natural slope, and the monoliths therefore had to sustain some horizontal thrust from the backing, as evidenced by the fact that they tilted forward slightly after the dredging in front had been done.

The retention of the original 45-foot monoliths had been governed largely by the circumstances obtaining at the time when the trouble developed with the sinking of the earlier ones. The contractor's yard was already laid out for 45-foot monolith blocks, nearly all the steel shoes were on the site, and a large number of the monoliths themselves had been sunk or were in the process of sinking. Furthermore, had the monoliths been made less than 45 feet square, the compensation-platform would have had to be made correspondingly wider.

The design of the wall had been carefully reviewed before work on the second stage was commenced, but it was considered that, having regard to all the circumstances, no economy would accrue by departing from the modified design as illustrated in *Figs. 8*, p. 194 §.

The maximum tilt had been observed in a portion of the modified section, where the slope had been grabbed out and the compensation-slab built prior to the removal of the material in front of the quay. The observations taken indicated that the movement was due to tilt and not to lateral displacement.

With regard to the slope behind the monoliths, the stone pitching was hand-packed above low water. Periodical observations still being taken had shown that no movement of the pitching was taking place.

§ *Ibid.*

Vessels were permitted to have a slow trial of engines whilst lying alongside the quay; particular attention was given to the slope where those trials were being carried out, and no movement had yet been observed.

The material of the gravel embankment was prevented from "spewing" under the capping beam by rubble stone packed behind the tops of the piles.

The difference between the cost of the wall built to the original design and that built to the modified design was very little, and for all practicable purposes they could be regarded as equal.

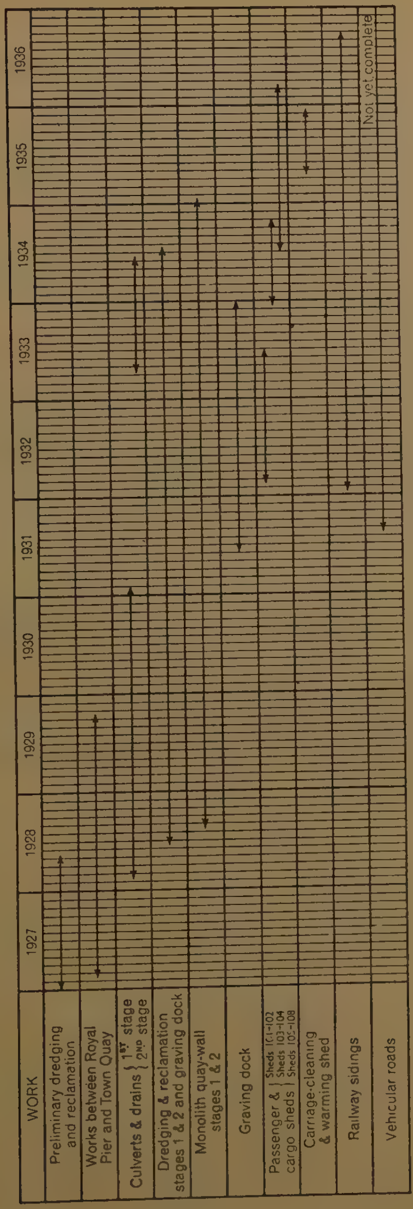
The sole reason for the change from the original to the modified design was the difficulty in sinking to the greater depth without damaging the monoliths. The result of the change was that toe-pressure was decreased and the heel-pressure increased to an extent which gave, in the modified design, almost uniform pressure over the whole of the base, but both designs could be said to be equally stable. An incidental advantage of the modified design was that it was quicker to construct.

Mr. E. J. Buckton gave a description of the quay wall recommended by his firm. That design had many merits, but it had not been adopted because, firstly, previous experience at Southampton had shown how very difficult it was to sink small monoliths through the gravel and sandy-clays which obtained there (and, indeed, it seemed that small monoliths could not be sunk to the depths shown in *Fig. 23*, p. 557, *ante*); and, secondly, owing to the great disturbance and softening of the surrounding ground which took place when monoliths were sunk through such strata, it seemed doubtful whether the 1-in-2 slope over a length of 40 feet between the monoliths, or even the monoliths themselves, would be stable.

Experience at Southampton had proved that timber-slides were very useful adjuncts to the graving docks, and they were used extensively for lowering to the dock-bottom a miscellania of articles such as drums of paint, materials for staging which had to be erected from time to time, oxygen and acetylene gas-cylinders, bilge-shores (in those docks where side shores were used and where bilge-blocks were not provided), etc. The use of the heavy-duty cranes alongside the docks would not be economical for handling such comparatively light articles, and, moreover, the cranes were not always available, being often occupied elsewhere in dealing with the heavier loads for which they were provided; waiting for the use of the cranes might cause delays which could be ill-afforded in the case of liners running on a rigid time-schedule, and where the time spent in dry-dock had perforce to be reduced to a minimum. Neither would the provision of lighter-duty cranes be an economical solution. Furthermore, in each of the timber-slides was incorporated a flight of steps extending from cope-level to dock-bottom, and those provided a ready and convenient means for the workmen to get to and from the dock-bottom.

The volume of water entering the graving dock from the permanent relief pipes was approximately 150 gallons per minute.

Fig. 28.



Progress-Diagram of Works.

It was considered that the life of the filters should be some 30 or 40 years. If at the end of that time it were thought desirable to continue their use, new wells would have to be sunk to replace them.

During the construction of the floor of the dock a large number of 2-inch-diameter pipes had been built in, extending to the underside of the floor and close to the transverse and longitudinal joints, with the object of grouting up any joints which might open up due to the contraction of the concrete. It had been found, however, that very little grout could be pumped into the pipes. At a later date a number of the joints had opened up, and then holes had been drilled in the floor to intersect the joints at a low level. Cement grout had again been pumped in, and by that means the shrinkage-cracks had been effectually sealed.

No serious trouble had been experienced as a result of the settlement on the reclaimed land. The shed-floors were on the site of the main gravel reclamation-bank where the contractor's temporary works railways had been situated while the quay-wall was being constructed. No doubt the traffic over the bank during that time had assisted in consolidating it, in addition to which the floor-areas had been thoroughly well rolled before the concrete was laid. The result had been that practically no settlement had taken place on the shed-floors. Elsewhere the railway lines had to be regularly attended to and packed up ; the vehicular roads had settled up to a maximum of about 12 inches, and, as stated in the Paper, the surface had been brought up to level where necessary with tar-macadam.

No detrimental effect on the approach-channel through the reduction of scour on the ebb tide had been observed.

The time-programme for the commencement and completion of works was given in *Fig. 28*.

ADDITIONAL ORIGINAL COMMUNICATIONS

RECEIVED BETWEEN THE 1ST SEPTEMBER, 1937, AND THE 31ST AUGUST, 1938.*

TITLES.

AERIAL SURVEY.—An Aerial Survey of the Estuary of the River Dee. J. L. Matheson. No. 5,157.

AIRPORTS.—The Singapore Airport. R. L. Nunn. No. 5,193.

BRIDGES.—On the Problem of Stiffened Suspension Bridges, and Its Treatment by Relaxation Methods. R. J. Atkinson and Prof. R. V. Southwell. No. 5,195.

Wind-Pressure Experiments at the Severn Bridge. A. Bailey and N. D. G. Vincent. No. 5,187.

Experiments on Stress-Distribution in Reinforced-Concrete Arches. R. H. Evans and I. G. Moore. No. 5,178.

An Experimental Study of the Voussoir Arch. Prof. A. J. Sutton Pippard and R. J. Ashby. No. 5,177.

The Loading of Interconnected Bridge Girders. Prof. A. J. Sutton Pippard and J. P. A. de Waele. No. 5,176.

Experiments on the Stability of the Self-Anchored Suspension Bridge. A. H. Toms. No. 5,166.

See also River-Training.

DAMS.—Model Experiments on Bellmouth and Siphon-Bellmouth Overflow Spillways. G. M. Binnie. No. 5,156.

The Gorge Dam. W. J. E. Binnie and H. J. F. Gourley. No. 5,188.

DOCKS AND HARBOURS.—Improvements at the Royal Docks, Port of London Authority. R. R. Liddell. No. 5,184.

DREDGING.—The Principles of Drag-Suction Dredging. Herbert Chatley. No. 5,152.

HIGHWAYS.—Highway Engineering Practice in the United States of America. R. C. T. Allen. No. 5,160.

HYDRAULICS.—A New Theory of Turbulent Flow in Liquids of Small Viscosity. Thomas Blench. No. 5,185.

Considerations on Flow in Large Pipes, Conduits, Tunnels, Bends, and Siphons. James Williamson. No. 5,189.

Wave-Formation in Regulating Sluices. Hassan Zaky. No. 5,181.

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* Available for reference in the Library; includes Papers awaiting publication.

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Schemes of Improvement for the Cheshire Dee: An Investigation by Means of Model Experiments. Jack Allen. No. 5,194.

PATHOLOGY.—Anti-Malarial Operations in the Delhi Urban Area. A. W. H. Dean. No. 5,190.

POWER AND POWER TRANSMISSION.—HYDRO-ELECTRIC.—The Construction of the Uhl River Undertaking. H. P. Thomas. No. 5,183.

—— STEAM-ELECTRIC.—Suggested Basis of Comparison for the Efficiency of Steam Turbo-Generators and of Steam-Electric Generating Stations. J. F. Field. No. 5,186.

RAILWAYS.—Some Experiments on the Lateral Oscillation of Railway Vehicles. R. D. Davies. No. 5,158.

Railway Track Work for High Speeds. J. Taylor Thompson. No. 5,180.

RIVER-TRAINING.—The Principles of River-Training for Railway Bridges, and Their Application to the Case of the Hardinge Bridge over the Lower Ganges at Sara. Sir Robert R. Gales. No. 5,167.

STRENGTH OF MATERIALS AND STRUCTURES.—The Properties of Composite Beams, Consisting of Steel Joists encased in Concrete, under Direct and Sustained Loading. Prof. Cyril Batho, S. D. Lash and R. H. Kirkham. No. 5,179.

Indeterminate Structures: A New and Easy Mechanical Solution. Prof. J. A. Taraporevala. No. 5,182.

WATER-SUPPLY.—The Water-Supply of Kumasi in the Gold Coast Colony. C. Wilson Brown and C. L. Howard Humphreys. No. 5,191.

Subterranean Sources of Water in the City of Rangoon. H. C. E. Cherry. No. 5,192.

Gunite Lining of the Channel Conveying the Water-Supply of Lashkar City, Gwalior, from the Reservoir to the Filters. A. R. Pollard and D. C. Baxter. No. 5,153.

Reservoir-Repairs under the Reservoir (Safety Provisions) Act, 1930. R. C. S. Walters. No. 5,155.

ENGINEERING RESEARCH.

COMMITTEE ON THE DETERIORATION OF STRUCTURES IN SEA-WATER.

The Seventeenth (Interim) Report [1] of the Committee of The Institution of Civil Engineers appointed to investigate the Deterioration of Structures of Timber, Metal and Concrete Exposed to the Action of Sea Water has recently been published. This Report contains a general description of the work done during the past year, a summary of the work on the protection of timber during the previous year, and notes by Professor George Barger on the resistance of timbers; particulars are given by Dr. J. Newton Friend of the losses in weight and pitting of the Committee's iron and steel specimens after 15 years' exposure. A report for the year 1936–37 by Dr. R. E. Stradling on the experiments on the deterioration of reinforced concrete, and a note by Dr. Carl Benedicks on the iron and steel tests, are also included.

The report is of particular interest in that the complete quantitative results of the 5-, 10-, and 15-year experiments on the corrosion of iron and steel are given. These tests are now completed, but it has been necessary to defer a full discussion of the results till the next (Eighteenth) Report is issued.

THE INSTITUTION RESEARCH COMMITTEE.

Sub-Committee on Breathing Apparatus for Use in Sewers.

This Sub-Committee, formed in November 1935, has now submitted its report: "Recommendations in regard to Breathing Apparatus for Use in Sewers." This has been published by The Institution and may be obtained from Messrs. William Clowes and Sons, Limited, 94 Jermyn Street, London, S.W.1, at a price of 6*d.* to members and 1*s.* 6*d.* to non-members, post free.

The Sub-Committee (which includes members nominated by the Home Office, the Mines Department, the Institution of Municipal and County Engineers, and the Federation of Civil Engineering Contractors), after examining existing types of breathing apparatus and considering what modifications were desirable for work in sewers, carried out tests at Birmingham University, as a result of which it has been possible to specify the requirements for a suitable breathing apparatus.

[1] The Report is published for The Institution of Civil Engineers by His Majesty's Stationery Office under the authority of the Department of Scientific and Industrial Research; copies, price 9*d.*, may be obtained from H.M. Stationery Office, or through any bookseller.

Sub-Committee on Reinforced-Concrete Structures for the Storage of Liquids.

This Sub-Committee has now submitted its report in the form of a "Code of Practice for the Design and Construction of Reinforced-Concrete Structures for the Storage of Liquids," which has now been published by The Institution, copies being obtainable from Messrs. William Clowes and Sons, Limited, price to members 1*s*. 6*d*. and to non-members 2*s*. 6*d*., post free.

It will be remembered that the report of the Reinforced Concrete Structures Committee of the Building Research Board, issued in 1934, contained a Code of Practice for the use of Reinforced Concrete in Buildings, structures for the storage of liquids being expressly omitted from consideration. It was the need for guidance in respect of such structures that led to the formation of the Sub-Committee in June 1935; the Institution of Municipal and County Engineers, the Institution of Structural Engineers, and the Institution of Water Engineers co-operated by nominating representatives.

The Code contains an introduction in which the essential differences in the requirements for reinforced-concrete structures for the storage of liquids from those of other reinforced-concrete structures are dealt with, namely, the need for imperviousness, the avoidance of cracking, and the need to ensure that the liquid contained should have no corrosive effect on the concrete. This is followed by four sections, section 1 dealing with the scope of the report and general matters, section 2 with materials up to the point at which construction commences, section 3 with design, and section 4 with construction. Numerous Appendixes are also given.

Extracts from the D.S.I.R. Code for Buildings have been included so as to make the present Code self-contained. Such sections are indicated, thus emphasizing the further requirements necessary in the case of structures for the storage of liquids.

Sub-Committee on Earthing to Metal Water-Pipes and Water-Mains.

The report of this Sub-Committee has now been published by The Institution, and copies may be obtained from Messrs. William Clowes and Sons, Limited, price 6*d*. post free.

It consists of regulations for controlling the earthing of electrical installations to metal water-pipes and water-mains; in view of their importance, the full text is given below.

REGULATIONS FOR CONTROLLING THE EARTHING OF ELECTRICAL INSTALLATIONS TO METAL WATER-PIPES AND WATER-MAINS,

drawn up and approved by

THE INSTITUTION OF CIVIL ENGINEERS,
THE INSTITUTION OF ELECTRICAL ENGINEERS,
THE INSTITUTION OF WATER ENGINEERS,
THE BRITISH WATERWORKS ASSOCIATION, AND
THE WATER COMPANIES' ASSOCIATION.

Introduction.

IT has for many years been a common practice to utilize incoming water-mains for the earthing of electrical installations.[1] Cases have arisen, however, where corrosion of water-mains has been attributed to such earthing. It is known, for example, that continuous current causes electrolytic corrosion where it leaves a metal conductor or pipe to enter the earth, but the effect of the passage of alternating current to earth is somewhat obscure. A further point in connexion with earthing is that there have been instances where electric shocks have been incurred during the repair of water-mains.

It was obvious that some agreement as to the conditions under which earthing-connexions should be made was desirable, and the question was considered by a Joint Committee of the Institution of Electrical Engineers and the Metropolitan Water Board, as a result of which a draft memorandum on Earthing to Water-Mains was issued on the 20th December, 1926. It was not possible, however, to conclude the negotiations on that occasion.

The need for such agreement became increasingly evident, and it was in these circumstances that a Sub-Committee of the Institution of Civil Engineers Research Committee was formed in March 1936, to explore the problem of possible injury to metal water-pipes and mains through the earthing thereto of electrical installations, particularly in relation to alternating currents, with a view to

(*a*) investigating the existence and extent of such injury, research being carried out if necessary;

(*b*) obtaining mutual agreement on the conditions under which earthing-connexions to metal water-pipes and water-mains might be made; and

(*c*) if necessary, formulating a set of regulations in respect thereof.

[1] As defined by the Electricity Supply Regulations, 1937, for Securing the Safety of the Public . . . Clause 29 (*a*) (i): i.e. All metal work enclosing supporting or associated with the consumer's installation, other than that designed to serve as a conductor.

The Sub-Committee includes nominees of the Institution of Electrical Engineers, the Institution of Water Engineers, the British Waterworks Association and the Water Companies' Association, and acknowledgement is here made of the valuable assistance which they have rendered.

The Sub-Committee have taken into consideration the memorandum drawn up by the above Joint Committee, and they have been successful in securing unanimous agreement on the conditions under which earthing-connexions may be made in recommending the following Regulations in respect thereof.

The Regulations are intended to be applicable generally, with one exception: Post Office installations other than those for power and lighting services are excluded from the application of these Regulations, but are subject to agreement between the authorities concerned.

In the course of their work the Sub-Committee came to the conclusion that there were certain other aspects of the problem which could with advantage form the basis of research. The Institution gratefully acknowledges the funds which have been subscribed for such research by the interests concerned, and the investigation has been undertaken by the British Electrical and Allied Industries Research Association. The programme of research so far approved is as follows :—

(i) The amount and effect of aggregate leakage-currents on water-pipes.

(ii) The possibility of partial rectification of alternating currents in underground water-supply systems

(*a*) at earthing-connexions;

(*b*) between metal pipes and the soil.

(iii) The possibility of primary-cell effects in water-supply systems.

(iv) The relation of the above to the question of corrosion.

REGULATIONS.

Preamble.

These Regulations have been drafted under the auspices of the Institution of Civil Engineers as the result of agreement come to between representatives of Water and Electrical Interests. They are subject to any amendment which may be shown to be desirable as a result of further experience or research.

Clause 1.

An earth-wire connecting an electrical installation to a water-main or water-pipe is to be used only :—

(*a*) as a measure of safety for the purpose of returning to the source of supply such leakage current as may flow, or result from a failure of insulation.

(*b*) for radio-frequency currents and those from radio-interference-suppression devices.

Clause 2.

A water-main or water-pipe shall not be cut, drilled or broken, for purposes of Clause 1, and all reasonable and proper care shall be exercised, in making any earth-connexion, to prevent injury or damage to a water-main or water-pipe.

Clause 3.

Every earth-connecting device to a water-main or water-pipe shall be of such an approved design [1] as to ensure an efficient electrical connexion, and other than as provided for in Clause 4 shall be attached in a position convenient for, and easy of, access.

Clause 4.

An earth-connexion shall only be made to a buried water-main or water-pipe after notice to, and in a manner approved by, the water authority concerned.

Clause 5.

Wherever an earth-connexion is made to a water-main or water-pipe on any premises in which is installed a water-meter, a proper, sufficient, and suitable bond shall in all such cases be placed across such water-meter by the user of the meter, free of expense to the water authority.

Clause 6.

Where the water-supply authority has reason to believe that damage to water-mains or water-pipes is being caused by an excessive flow of current from an earth-connexion made to a water-main or water-pipe they shall, in general, request the electricity-supply undertakers for the district to test the installation, arrangements being made for a representative of the water-supply authority to be present at the time the test is made. If, however, for any reason the water-supply authority should desire to test for electrical leakage from an installation to water-mains or water-pipes, that authority will be at liberty to make such test after advising the electricity-supply undertakers for the district of their intention, giving such notice to the consumer as may be necessary, and inviting the presence of a representative of the electrical undertakers when the test is made.

Water-supply authorities (whilst maintaining the powers which they are advised are conferred by existing water-supply legislation to enter premises,

[1] For the purposes of Clause 3 the approval of the design of the earth-connecting device should rest with a joint committee of electrical and water representatives.

and if necessary to test for electrical leakage) agree that, in general, tests for electrical leakage, and any notice to the consumer which may be necessary in connexion therewith, should be made and given by the electricity-supply undertakers, who will usually possess the better facilities.

NOTE.—Attention is drawn to the fact that in certain cases non-metallic water-pipes are in use, and the electrical implications of this should be recognized.

The constitution of the Sub-Committee is as follows:

S. B. Donkin (*Chairman*) H. J. F. Gourley, M. Eng. R. G. Hetherington, C.B., O.B.E., M.A. E. F. Law H. W. Swann	Of the Home Office.
Ll. B. Atkinson Percy Dunsheath, O.B.E., M.A., D.Sc. F. W. Purse P. J. Ridd	Representing the Institution of Electrical Engineers.
H. F. Cronin, M.C., B.Sc. B. W. Davies	Representing the Institution of Water Engineers.
F. J. Dixon R. W. James	Representing the British Waterworks Association.
J. D. K. Restler, O.B.E.	Representing the Water Companies' Association.

Secretary to the Sub-Committee: A. H. Naylor, M.Sc.

REPORT OF THE CHEMISTRY RESEARCH BOARD.[1]

Research undertaken at the Chemical Research Laboratory, Teddington, is reviewed in the Report[1] for the triennial period ended 31st December, 1937. A few of the investigations described are discussed briefly below.

An investigation into the corrosion of metals, both in the air and in immersed conditions, includes a prolonged inquiry into fundamental causes. Concurrently with this long-range research, many investigations on technical problems, such as the corrosion of locomotive boiler-tubes and fire-extinguishers, have been carried out for industry. The possibility of more widespread use of light magnesium alloys for industrial purposes

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[1] Published by H.M. Stationery Office, price 3*s*. net.

adds interest to the results achieved in the protection of these alloys against attack by sea-water and by leaded petrol fuels ; in the former case a useful degree of protection has been attained by a chemically-deposited coating of selenium, and in the latter case corrosion has been entirely suppressed by the addition of a small proportion of a harmless inhibitor to the fuel.

Bacterial investigations have been made in connexion with several diverse problems, including the anaerobic corrosion of iron ; in this case, it is considered that sulphate-reducing bacteria are responsible, the affected areas of cast-iron pipes becoming denuded of iron and reduced to a soft matrix of graphite.

Considerable progress is reported in the theory and technique of chemical reactions at high pressures and temperatures, including the hydrogenation of coal to oil.

A research on the constitution of coal has recently been initiated, and a comprehensive study of coal tars, especially those derived from low-temperature carbonization, has added greatly to the knowledge of tar-constituents. It is hoped that the research may lead to the commercial production of catechol in Great Britain. The causes and prevention of corrosion in tar-stills haye been studied.

The treatment and utilization of rubber have been studied ; chlorinated rubber has been found to have very interesting and useful properties.

Other researches in which considerable progress has been made include synthetic resins, rare metals, and therapeutic products.

REPORT OF THE FOREST PRODUCTS RESEARCH BOARD.

The following brief notes refer to the recently-published Report[1] of the Forest Products Research Board for the year 1937.

The policy of holding an annual Summer School has been amply justified.

The moisture-content of timber, and its bearing upon seasoning, have been further studied ; special attention to seasoning is necessary in the case of timber for use in centrally-heated buildings. An improved form of seasoning kiln has been developed. The moisture-content of timber in the National Gallery has been found to fluctuate between $9\frac{1}{2}$ and $13\frac{1}{2}$ per cent. The moisture-resisting qualities of paints on wood have been investigated.

An examination of the 4,400 railway-sleepers which were laid in the tracks of the main railway companies during 1935 indicated that the amount of splitting occurring in the 2-year period varied widely ; it was less pronounced in sleepers laid with the heart face up than in those laid heart face down, and was much less in incised sleepers.

[1] Published by H.M. Stationery Office, price 2*s.* net.

Woodworking and bending processes have been extensively studied. The desirable characteristics of woods for bending have been determined; it is pointed out that most woods can be bent across the grain at least as readily as along the grain.

The reflexion of light from wood surfaces has been studied in connexion with the decorative uses of wood. The correct placing of the wood in relation to the direction of illumination is essential if the best results are to be obtained, and the " figure " of a wood is brought out by attention to this point.

Much progress has recently been made in wood products such as ply-wood, compressed and impregnated woods, and combinations of veneers with metal, asbestos, fabric and bakelite. Researches have been begun from which important developments of these new materials may be anticipated.

Other work includes studies of tree-growth and timber-quality, box-testing, fire-resistance, preservatives, wood-rots and borers, and glues and adhesives. A great deal of advisory work is carried out for commercial and other outside organizations; for example, over five hundred enquiries on seasoning and bending problems alone were dealt with during the year.

FIFTH REPORT OF THE CORROSION COMMITTEE OF THE IRON AND STEEL INSTITUTE.

This Report, which has recently been published[1], gives the present position of the Committee's work in connexion with atmospheric corrosion and marine corrosion. The research consists of field tests, associated laboratory tests, and fundamental work, and includes consideration of protective coatings.

In connexion with atmospheric corrosion the variation in the amount of corrosion of unprotected iron and steel is correlated with locality, and the state of atmospheric pollution is discussed. It is noted that the rate tends to decrease with time. The importance of the character of the rolling scale is emphasized, and also the effect of the amount of slag-inclusion in wrought iron. The maximum reduction in corrosion resulting from the addition of small amounts of alloying elements such as copper and chromium was found to be about 40 per cent. The corrosion of railway-sleepers of copper-bearing steel, the effect of methods of application of paint, and the use of metallic coatings, are also discussed.

In connexion with marine corrosion, de-scaling prior to initial painting is recommended. Maintenance-questions and the effect of electrical leakage are also dealt with.

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[1] Published by the Iron and Steel Institute, 28 Victoria Street, S.W.1, price 16*s*.

RESEARCH WORK IN ENGINEERING AT GLASGOW UNIVERSITY.

July, 1938.

The following note indicates briefly the nature of the researches in progress in civil, mechanical, and aeronautical engineering.

Roads and Railways.

A study that has been made of the most suitable curves for roads and railways indicates the value of the clothoid[1], and Tables have been prepared for the setting-out of this curve in the field.

Hydraulics.

A comparison has been made of the actual and theoretical positions of the hydraulic jump in a channel[2].

Materials and Structures.

An investigation is being made of the fundamental laws governing the behaviour of metals in the plastic state, the relation between stresses and strain, and the progress of strain-hardening.

The fatigue of steel strained plastically to failure in alternating bending is being studied, and the results compared with the tensile, Izod and normal fatigue values.

A study is being made of the tendency of welds to crack on cooling when using different electrodes and procedures, with a view to arriving at a standard method of test.

The arithmetical method of solution of equations of the type $\nabla^4 w =$ const. is being applied to the flexure of flat plates loaded in various ways by concentrated or distributed normal forces with different kinds of edge-support, and theoretical solutions are being compared with experimental results. It is hoped to extend the work to reinforced-concrete slabs and to include the case of slabs with an intermediate support.

Heat-Engines.

A study is being made of combustion in oil-engines, including an examination of the time-lag in ignition of fuel-oils over an extended range of pressure and temperature. An experimental high-speed oil-engine is being adapted for studying certain combustion-conditions in relation to performance.

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1 A. Thom, "Standard Tables and Formulæ for Setting Out Road Spirals." London, 1935.

2 ——, *Correspondence on* "The Flow of Water in Short Channels," by C. F. J. Lisle, *ante*, p. 427.

The pressure-variations in, and noise from, exhaust-pipes and manifolds of internal-combustion engines for different lengths and bores of pipe, valve-sizes, and timing and silencing arrangements are being investigated, with a view to their reduction, while maintaining high volumetric efficiency and compactness of form.

A research is being carried out into the combustion and flow of gases in a gas turbine of the intermittent-flow type, and the effect of variations in the timing of explosion and valve-opening is being studied.

A study has been made of the single-stage steam-operated air-ejector, discharging to the atmosphere from a suction-chamber to which air is admitted at varying rates. The effects of position of steam-nozzle, divergence and length of diffuser, and other variables have been investigated.

A small experimental single-stage centrifugal air-blower is being tested to find how its efficiency and certain of its other characteristics vary with the Reynolds number corresponding to flow through the machine.

Aeronautics.

The interpolational arithmetical method of solution has been applied to certain hydrodynamic problems.

The effect of disks on the aerodynamic forces on a rotating cylinder has been studied in a wind-tunnel. Other wind-tunnel work includes heat-flow and evaporation from surfaces in an air current, and the determination of drag by the pitot-tube-traverse method.

A channel has been developed for the production of a steady uniform flow of oil or water, and the forces on an aerofoil at very low values of the Reynolds number and during acceleration have been studied.

The above researches have been carried out at Glasgow University under the direction of Professor Gilbert Cook, D.Sc., Regius Professor of Engineering, Professor W. J. Goudie, D.Sc., Professor of Heat Engines, and members of the Staff.

NOTES ON RESEARCH PUBLICATIONS.

1. Engineering Construction.

(2) *Surveying.*

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(2) *Engineering Physics.*

(a) *Elasticity and other mechanical properties.*

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The elastic stability of a curved plate under axial thrusts, *J. Roy. Aero. Soc.*, **42**, *536*.

Supersonic measurement of elastic constants of solids, *Helvetica Physica Acta, 1938*, **11**, *(2) 113*.

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A rapid method for the determination of principal stresses across sections of symmetry from photo-elastic data, *J. Applied Mechanics, 1938*, **5**, *(1)* *(A24)*.

Photo-elastic analysis of stress concentration for beams in pure bending with a central hole, *J. Franklin Ins. 1938*, **225**, *(5) 515.*

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The failure of girders under repeated stresses, *J. Inst. C.E.*, **9**, *301* (Eng. Abs. **1**, Con. *244*).

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(b) *Soil Mechanics.*

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The figure in heavy type is the number of the Volume; that in brackets the number of the Part; and that in italic type the number of the Page. In references to "Engineering Abstracts" the number of the Volume is given in heavy type, the section is indicated by the abbreviation Con., Mech., Ship., or Min., and the number of the Abstract is printed in italic type. The scheme of tabulation is given in the January, 1938, Journal (pp. 475–477), to which reference should be made.

* When it is known that reference will appear in an early issue of "Engineering Abstracts" this fact is indicated by an asterisk.

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Calculations for structures resting on elastic foundations, *Tech. Travaux* **14**, *323* (Eng. Abs. **1**, Con. *270*).
Horizontal pressures on retaining walls due to concentrated surface loads, *Iowa Eng. Exp. St. Bull.*, *140* (Eng. Abs. **1**, Con. *266*).

(c) *Mechanics of Fluids.*

Concerning secondary flow in straight pipes, *Proc. Cambridge Phil. Soc.*, **34**, *335*.
Pressure losses resulting from changes in cross-sectional area in air ducts, *Univ. Ill. Eng. Expt. Stn. Bull.*, **35**, *52*.
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(3) *Operations and Methods.*

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(4) *Structures.*

(b) *Dams, Retaining-Walls, etc.*

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(d) *Bridges, Arches, Roofs, Hangars, Framed Structures, etc.*

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(f) *Buildings.*

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OBITUARY.

SIR JOHN DEWRANCE, G.B.E., the only son of John Dewrance, erector of George Stephenson's "Rocket," was born at Peckham on the 13th March, 1858, and died on the 7th October, 1937, at Wretham Hall, near Thetford. He was educated at Charterhouse and at King's College, London, of which he was made a Fellow in later life. He served as a pupil to Colonel John Davis, Assoc. M. Inst. C.E., at the works of Messrs. Dewrance & Co., becoming a partner in 1879. In 1880, as a separate venture, he took over the research laboratory and staff of Professor Barff, eventually developing it into the Albion Chemical Company. In the laboratory he carried out extensive investigations of the problems of lubrication, the corrosion of boilers, the composition of bearing metals, and also the production of aluminium. In 1899 he was elected Chairman of Messrs Babcock and Wilcox, Ltd., and held this position until July, 1937, when he retired.

He was actively interested in the development of the Kentish Coalfields. During the war he served on various committees of the Ministry of Munitions and the Ministry of Labour, and also on a Treasury Committee. In 1920 he was awarded the K.B.E., and 8 years later the G.B.E.

He was elected an Associate Member of The Institution in 1884 and was transferred to the Class of Member in 1899. He was President of the Institution of Mechanical Engineers in 1923 [1], and of the Institute of Metals in 1926–28. He was awarded a Telford Premium for his Paper on "Machinery Bearings" [2] and a Watt Gold Medal and a Telford Premium for his Paper on "The Corrosion of Marine Boilers." [3]

Sir John married Isabella Trevithick, the granddaughter of Richard Trevithick, by whom he had one son and one daughter.

ALEXANDER FORRESTER STEWART was born at Black River, Richmond, N.S., on the 8th January, 1864, and died at Halifax, N.S., Canada, on the 30th October, 1937. He was educated at Pictou Academy, N.S., and Dalhousie University, and received his scientific training at McGill University. He entered the service of the Canadian Pacific Railway in 1887 and worked on pioneer railway construction in the west. In 1895 he went to South Africa, where he remained 11 years. He was at first employed on surveys and construction in Natal, Transvaal, Zululand, and Cape Colony, while during the Boer War he was in the service of the Imperial Military Railways in the Transvaal.

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1 See Memoir, Proc. I. Mech. E., vol. 136 (1937), p. 396.
2 Minutes of Proceedings Inst. C.E., vol. cxxv (1895–96, Part III), p. 351.
3 *Ibid.*, vol. cxli (1899–1900, Part III), p. 107.

From 1903 he was employed on surveys and maintenance for the Cape Government Railways until he returned to Canada at the end of 1906. He was then with the Canadian Northern Railway until its incorporation into the Canadian National System. He retired in 1932, after he had been appointed Chief Engineer of the Canadian National Railway's Atlantic division in 1920.

He was elected a Member of The Institution in 1910, and acted as Member of Council for Canada from 1931 to 1933.

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